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PSYCHOLOGICAL REVIEW PUBLICATIONS

Psychological Review

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THE PSYCHOLOGICAL REVIEW

THE MAJOR CATEGORIES OF PSYCHOLOGY[1]

BY MADISON BENTLEY

University of Illinois

When the banqueters had drained their coffee-cups and lighted their tobacco, the local committee on entertainments and penalties announced that Station BABL, renowned both for its lofty tower and for its long polyglottic service, was on the air. Immediately there fell upon unprotected ears the crash and crackle of a thousand battles, succeeded by a single wailing and piercing whistle as from a distant desert hurricane. Suddenly the sand-blast took voice. 'Go to,' it commanded, 'let us go down, and there confound their language, that they may not understand one another's speech.' Thereupon confusion reigned. BABL outdid Bedlam. Only occasionally did a coherent phrase leak through: 'And man became a living *soul*,' 'No one cared for my *soul*,' 'I will *behave* myself wisely in a perfect way,' 'Confounded be all they who serve . . . *images*,' 'I would that ye knew what great *conflict* I have.' While the committee patiently stood by, trying again and again to clear the air and to recover the lost wave, the guests offered desultory comments. 'Just my luck,' sighed one. 'Never yet have I had a coherent half-hour from this station.' 'Possibly you are not versed in the languages of BABL,' suggested Phoneticus. 'It isn't that,' put in Spinoza, who was deftly teaching a new card trick to a group of nice Ph.D. boys seated at his table. 'It isn't that, in my case. I converse in all languages; behaviorism, mentalism, structuralism, freudism, objectivism, configurism and even anthroponomy. But I simply don't get it.' 'Leave

[1] Presidential address delivered before the American Psychological Association at Ithaca, December 29, 1925.

off your card tricks,' snapped Professor Morrill Sage with authority. 'They may possibly be ethical; but 'pon my word they are not attributive.' 'Process server,' retorted Spinoza, 'your view is always narrow. As for me, I regard all things *sub specie æteritatis*.' '*Specie*, yes,' shouted Professor Sage; '*specie audiences*'! 'One complex is as bad as another,' interjected Wattsavior. 'If you gentlemen were to forget your moral and attributive souls and perfect your behavior, you might learn to condition your unseemly viscera.' 'Yes,' added Christopher Wise von Columbo, 'let us forget that we are not of the social class of those who introspect and have sensations. Let us react modestly and biosocially to all verbal stimuli. The representatives of the press, noting our visceral responses, will think us more *freudlich* than *freundlich*, and so depict us adrenally in the papers.' 'I am *always* friendly,' returned Spinoza, 'at least subconsciously; but I insist upon distinguishing fact from fable. Only the other day I was addressing a large group of federated ladies ——.' 'Why barter differences?' broke in a hollow voice from the ether. 'I will radio a questionary and thus settle at once the matter at dispute.' 'I wonder,' queried Damascus Steelburn, 'whether he means to propound his questions in viennese, behaviorese, or recapitulese. It occurs to me that we need less dialectical talking and more diacritical thinking to compose our differences.' 'Talking and thinking are all one to me,' confessed Wattsavior—('So we have observed,' put in Dr. Steelburn subvocally.) —'though I have recently noticed in my business,' continued Watts, 'that the hand is the silent but eloquent partner in talking and that gastric twinges and intestinal slips help quite a lot too. I now realize and proclaim to you that flirting, for example, includes both laryngeal and visceral accidents and incidents.' Here he lapsed into a turgid, sacral form of thinking, of extremely short wave and high amplitude, which only the more highly sensitized guests could intercept. Presently Morrill Sage arose, recovered a black cigar from his beard, and proposed to distinguish once and for all pure technology from the true words of the Prophet of Leipzig. But his words were lost in a whistling sputter

from the antennæ of the desert. It was obvious to all that the ether was behaving badly and that the messages from BABL were coming through in a weak, 'implicit' form, wholly wanting in cognitive clearness.

For one moment only did the ceiling cease its convulsions; then came out of the East the vibrant voice of a mighty Stagirite. "With what ease do you men make yourselves known? Led by the light of the Maeonian star I wrote not only psychology but also politics, logic, linguistics, ethics, metaphysics, comedy, tragedy, physics, physiology, astronomy, botany, cosmology, evolution, zoölogy, economics, and much besides. Even so my fame rose slowly. Could I but have a decade for sport and notoriety in your age of advertising and golf! Why, the rearrangement of a single test would now put my name upon the billboards, and were I industrious enough to contrive an entire set or battery of tests, I might add to the Pressites, Binetites, Downeyites, Thornydikes and Termites the immortal name of the Aristotites."

The diversion was only momentary. It was obvious that the confusion of tongues had stirred the suprarenals of the guests. All were eager to 'respond,' each in his own chosen dialect. The situation was critical. Gastric functions were thrown out of commission. Blood pressures advanced. War was declared between the thoracic-lumbar and the sympathetic forces. Pillsarbor, the peacemaker, arose to save the occasion and to recompose digestive behavior. 'Why not cast lots,' he proposed, 'and allow one person only to speak? Then as we meet again and again, from year to year, others shall speak. Thus each dialect shall be represented and each dog (to draw an illustration from canine psychology) shall have his day. The proposal was applauded by all the pacifists present; though Professor Sage contended that practically the same method of choice had (barring local politics) prevailed for many years. 'Just see,' he exclaimed, 'what we have sometimes drawn, while the very best people have been overlooked.' General assent was finally gained, however; for so eager to speak in turn was each guest and so attractive

were the allurements of chance that the dice were called for and the lot fell out. Resigning themselves to an hour of boredom the guests re-lighted pipes, sighed, and proceeded to listen, to digest, or to prepare their respective articles of faith against the lucky turn of future fortunes.

The opinion is abroad—began in a rambling and hesitant manner the person upon whom the gods had smiled—the opinion is abroad that old things in psychology have passed away and that only new facts, new principles, and new methods are henceforth to be tolerated. He who runs a racing car may read the signs of the psychological times: 'The new psychology,' 'Complete behaviorism at last,' 'Freud brought up to date,' 'The latest revision of the libido,' 'The practical psychology of human drives and taxi-drivers,' 'The definitive measures of human intelligence,' 'New conquests of psychology among the Pullman porters,' and the like.

Now you may have observed, continued the speaker, that these repetitive farewells to the past which have spread themselves for twenty years over the pages of our literature suggest strong emotion. Men have denied with an oath that they ever knew the old psychology, now led away to ignominy. A single oath has not sufficed; they have iterated and reiterated their final breach with traditions, condemning and cursing the old in the roundest phrase. There is in this passionate denunciation just a hint that the break is not so irrevocable as the heralds of a new day would like to believe. Emotion may here cover an uncertain hope that anathemas and curses may serve to keep the dead past dead. We are reminded of Jean-Christophe at the violent end of one of his amorous episodes. He was said to be free of his old companion; 'but he was not free of himself.' The past lived in him. He could not shake it off.

But suppose we grant that the 'traditional psychology' (as the common phrase runs) is dead and gone,

> "At his head a grass-green turf,
> At his heels a stone."

Decent respect still permits us to comment at the grave upon the accomplishments of the departed. We may even be pardoned if we seek, by a comparison of the virtues of the dead and of the living, to estimate our gains. Thus may our sorrow be fittingly confined, for *excessive* grief is 'the enemy to the living.'

Justly to estimate the claims of rival doctrines and opposed points of view requires us to search out, if we can, the most fundamental concepts or categories upon which the contesting doctrines are built. By substituting in our discussion these major categories for individual systems and their expounders we may at once avoid prejudice and emotional bias and discover how permanently and broadly we may hope to rear our superstructures.

The present sharp antithesis among the fundamental concepts of psychology calls for serious remark, for it is not usual for any established science to sustain radically divergent views upon the essential nature of its material. Diverse views upon method and procedure we frequently find, as well as disagreements upon the ultimate use or the outside relations of a discipline; but it is rare to find antithetical positions held upon the very objects and processes which compose the fabric of a science. Now this is virtually the state of psychology today. Our consideration of basal categories has to begin with the recognition of the disturbing fact that three radically different conceptions of the material to be woven into the psychological system are not only current but are actively and aggressively espoused. Let us examine in turn the underlying conceptions which come to expression in three competing views.

Our first category is sometimes written down as the category of *objectivity*, a term which implies, in this connection, that the factual materials of psychology are to be sought among the objects and activities of the physical world. It is, as you will observe, a practical, not a logical, objectivity; for it fails to demonstrate that all objects are of the physical sort. It assumes instead that any subject-matter which is not 'objective' is 'subjective' in the sense of implying an alleged soul, mind or consciousness.

The most feasible way of dealing with this category seems to be through the form of it most widely current. This form is found in a variety of behaviorism, a doctrine which (as Woodworth[2] has explained) is by no means coincident with objectivity at large. It is rather one specific form of it,—that form, namely, which chiefly directs its attention to organic movements and which regards these movements as determined both as to cause and as to character by antecedent objects and agents. These objects and agents are the *cause* of the movement in the sense of a specific and parallel antecedent, and they *characterize* the movement by determining its psychological status. The antecedent and its consequent movement are commonly conjoined as *stimulus-response*, a verbal couple which expresses the most fundamental conception common to the varied forms of behaviorism and which implies a describable equivalence between something done to the organism and an answering or 'responding' activity of the organism itself. Thus the living organism is looked upon, not as an originator of its own activities, not so much as a separate physical system, but instead as a more or less passive intermediary. We may consider, in turn, the stimulus, the copula, and the response.

In the hands of one behaviorist, 'stimulus' is defined as a physical agent which sets up or changes excitatory processes in a receptor organ; and an 'effective stimulus' as any agent which calls forth a response. It is, by hypothesis, only the 'effective' stimulus which is considered by the behaviorist; and to make sure that stimulus and effective stimulus are practically coextensive, behaviorism adds to its assumption of the causal equivalence of stimulus and response the additional assumption (not always approved by non-behaviorists) that, barring 'resistance' in the nervous system, every stimulus gives rise to an 'effector' response. Beginning thus with the stimulus of the psychophysical metric methods (energy delivered at the receptor), the term has gradually been extended to the mass-objects of common sense, to bits of conversation

[2] Woodworth, R. S., 'Four Varieties of Behaviorism,' PSYCHOL. REV., 1924, 31, 258, 260.

(*e.g.*, 'meet me at the Belmont tomorrow for lunch at one o'clock'), to 'situations,' to occurrences, commands, suggestions, human traditions, and the descriptive records of the arts and sciences.[3] In short, the stimulus may be as wide as the undefined 'environment.' Thus a very great deal is included beside the immediate physical event at the receptor. For example, when a chair is seen, a symphony heard, or an invitation understood, the chair, the symphony, or the invitation jumps around the corner, so to say, and enters the organism as a stimulus. Whether this double-dealing of the environment involves the criminal fallacy of *hysteron-proteron* or only stands witness to the extreme naivété of the behaviorist, I leave undiscovered. It is a doubtful case of *hysteron-proteron*, for the cart is not simply placed before the horse. The horse (*i.e.*, the environment) is really behind and before, and it is actually one and the same horse! Since the organism turns out, so to say, only movements and secretions, it is not quite clear how the behaviorist legitimately comes by such things as chairs, symphonies and invitations; unless perchance he accepts them ready-made at the hand of a helpful creator. At the least, it seems necessary to add to the assumptions of the behaviorist one more which would permit him to label all these trappings of the universe 'stimulus.' That they may reasonably and sensibly be brought under the category of physical existence is usually assumed, not demonstrated, by the behaviorist. The universal applicability of the 'stimulus' label seems to require not logic but only devotion to the behavioristic faith.

In the second place, we must not overlook the wide variety in behavioristic views of the organic copula, as the physicist's 'missing link' which is supplied by psychology to unite, in the form of stimulus-and-response, the parted environment. In one writer the organism is an hypothetical array of neural pathways, equipped with opening and closing valves (a fantastic scheme of simplification which the neurologist himself

[3] A recent catholic list of 'stimuli' begins as follows: 'overthrow of monarchy, formation of Soviet government, war, prohibition, easy divorce, no marriage, children brought up in ignorance of their parents,' *etc.* (Watson, J. B., 'Behaviorism,' 1924, 32). With such stimuli, what psychologist could fail to be stimulating!

might now be glad to discard or, at the least, to revise).[4] Another writer chooses to neglect the neural coupling and to lay his emphasis upon the *development* of behavior; but here again the reflex is made the basal assumption and the modification of it (called 'conditioning') the chief accessory hypothesis, brought in to explain what is generally known as 'learning.' In this account, much stress is also laid of late upon the organization of visceral and vocal systems.

Finally, as regards 'response,' we may again call attention to current variations in treatment. As the early exponents tended to stress the raw physical agents in stimulus, so did they also regard the response as chiefly lying in the raw organic movement. In this form the doctrine suggests the epithet of 'muscle-twitching.' But elaborations of the theory have passed beyond the mere movement to its results. On the one hand, we have the revival of an old biological conception of adaptation and adjustment, a form of teleology which has proved acceptable to many behaviorists, appearing to gain scientific respectability by a divorcement from speculative philosophy for a union with speculative biology. As 'adaptation' and 'adjustment' are not, however, implied in our primary category, we may omit them from our discussion. A more integrative extension of behaviorism carries it to the immediate results of movement and secretion. I refer to such results as the manipulation of objects, the formation of words, 'overt' and 'implicit,' and the organization of a visceral 'environment,' as in the emotions and in the use of language. Here we have an extension of response beyond the organism which matches our earlier extension of stimulus.[5] Now both terms in our primary couple have been carried right into the environment, with the organism tending to approach a mere line of connection, the famous pathway-hypothesis retreating to the blackboard as little more than an arrangement for closing the inconvenient gap in the environment caused by the intervention of the non-conscious organ-

[4] Meyers, C. S., *Amer. J. Psychol.*, 1925, 36, 53–65.

[5] A list of responses complementary to the stimuli cited a moment ago includes such interesting human concerns as marriage, continence, joining the church, and truthfulness. (Watson, *op. cit.*, 33.)

ism. This tendency may represent an inclination to depart as far as possible from the abhorrent realm of the conscious. It is a curious outcome. Starting with the naïve assumption of a ready-made physical world, it ends with a polarization of that world into two 'environments,' the environment of stimulus and the environment of response, with the organism a sort of ineffective link between the two, which neither initiates nor changes. The chair, the symphony and the invitation go into the hopper as stimulus and come out again in response as chair handled, symphony visceralized and invitation accepted. There they are again, and the organism has incidentally twitched its striped and smooth muscles, exercised its voice, and emptied its glands. This tendency to neglect the organism is not only curious. It is also interesting. We might regard it as incidental to a general trend of the times toward objectivity, toward descriptive poetry and music, depictive painting and sculpture, and a quickened interest in the geography of the earth and of the galactic universe. I am not sure of the generalization; but at least the tendency in behaviorism is, as I have said, interesting. It may be that generations of biology have made us too self-centered for coherent description of our own organisms and that we need a reaction toward an environmental form of objectivity.

A still further extension of behaviorism, on the side of response and in the direction of the social disciplines, I can only touch upon. It appears in such books as Allport's 'Social Psychology' and Weiss's 'Theoretical Basis.' For Weiss the response is also a stimulus whenever it arouses a response in another individual. This strange transformation is made through the efficacy of the label 'biosocial.' Though defined as a form of movement, the response is thus allowed by this magical transformation to become a word and so to 'stimulate' another. Here naïve monism carries us a step further. In order to escape the bogs of the psychical, our author identifies the air-changes at the neighbor's ear with the luncheon invitation and so this invitation becomes not only the promise of a gay hour but also a (biosocialized)

stimulus. He verbally escapes the hateful problem of 'meaning' by using the word 'significance,' and the 'understanding' of the neighbor's generous intent by 'observing his social status.' Allport builds the psychological span of his bridge toward sociology by first declaring his faith in stimulus-and-response, denying that consciousness can accomplish so much as the bearing of its own weight; but then, when he arrives at his real problems in social psychology, he allows this inefficacious member to sustain practically the entire structure.

The historical circumstances under which the behavioristic category appeared are informing when we try to set this doctrine in its just perspective. I will venture to recall two or three of the incidents. In the nineties of the last century came the crystallization of the neurone theory. The nervous system was reduced to body cells which were regarded as linked together by way of their dendritic and axonal processes. Th[illegible]ch simple neural functions as those involved in the muscle-nerve integration were explained by means of this neuronal coupling, brought under the ancient concept of the reflex arc.[6] Carried up gradually from the spinal cord the reflex concept invaded the basal ganglia and the cerebrum, revising the doctrine of centers and threatening the concept of localization. It soon appeared that certain psychologies then current did not obviously accord with the doctrine of the reflex. But sense-centers were, by way of compromise, permitted to remain; although the logic of the arc demanded that they be regarded as nothing more than primary stations in the cortex for pathways on their circuitous journey from receptor to muscle. Then came Paul Flechsig with his notorious theory of the associational centers, and the task was done; sensations in the sense-centers and the associative binding of these in the rest of the cortex. What could be simpler! So strongly were the underlying concepts of the associational psychology intrenched that nearly everybody was satisfied.[7]

[6] Gault, R. H., *Amer. J. Psychol.*, 1904, 15, 526–568.

[7] Not quite everyone. Wundt needed his *Apperzeptionszentrum* and Münsterberg his *Aktionstheorie*. Both demanded a plan of integration not found in associationism.

At last psychology had a working model of the nervous system. Best satisfied of all were the budding behaviorists who grew up with the psychologized reflex arc. Their way was clear; straight up the cord, through the lower ganglia, through the sense centers, through the associative centers, and straight down (thanks to the 'motor' stimulation of Goltz and others) to tongue, toes and finger-tips. Of course no one then saw as plainly as we now perceive (behaviorists, of course, do not 'perceive') that the reflex arc and Flechsig fitted as by a pre-established harmony because they were one and the same doctrine; the bonding doctrine, that is to say, made famous by English empiricism. So at last the cherished doctrine of association became flesh. And here was behaviorism with its foot through the door. It only remained to declare that the bodily aspect of association be made all inclusive, binding the two end terms stimulus and response and casting off the conscious and epiphenomenal double, grown dry and scaly; a metamorphosis which changed only the material of associationism, not its form.

To be sure, other circumstances favored. First, the engaging reaction-experiment, a double bequest from physiology and astronomy, which kept many hands employed and gave a business-like air to the new laboratories. That was simple stimulus-and-response with a fine showing of mathematics in average times and mean variations. It is of course now clear that all the psychologists of those days who were only playing with movement-times as initiated by various forms of stimulus were behaviorists. Any description of the action-experience they scornfully avoided. Out of this set-up came then the association experiment, by which stimulus-word was immediately bonded or glued to response-word. Although it required no more than amateurish and half-hearted 'introspection' (as any bit of gossiping comment from a 'subject' is still called in some laboratories) to convince anyone who cared to see that much other stuff, verbal and non-verbal, was interwoven between the word-called and the 'response word.' Nor can we neglect, in this connection, the great prestige of evolutionary biology with its emphasis

upon adaptation and adjustment. There arose in its dignity the environment. It hurled its unfriendly self at the organism, and the organism had either to respond adjustively or to acknowledge its unfitness. Little wonder that it responded and that thereby the stimulus-response couple gained new significance.

Gradually therefore and from many beginnings came behaviorism, an unescapable issue. Its inevitable appearance at a ripe time suggests the reflection of the cockney traveler of forty years ago midway in his tedious journey of eight days and eight nights from the Pacific Coast to the Atlantic Seaboard. Strolling one evening as the train paused on the prairies he was heard to remark that he didn't think much of Christopher Columbus findin' this 'ere country. 'It's such a doose of a size I don't see how he could miss it.'

From the category of objectivity, examined in the special form of behaviorism, we turn to that of *experience*. This latter category is not to be regarded as the obverse of objectivity. Subjectivity is not its mode, and it does not imply by any necessity the concepts of consciousness and mind; although these concepts may, and in the hands of some psychologists they do, appear as a specific interpretation of the basal category of experience.

Under this fundamental category the locus of psychological problems lies in the biographical history of the individual. Human experience appears as the march of events regarded from the point of view of the living organism, the organism which apprehends objects, which keeps the run of the world's events, whether close or remote in space and in time; the organism which is seized by emotion, enthralled by craving and desire, enticed into understanding and absorbed by the processes and problems of thought. In order to distinguish the organism's part in experience and to bring into focus for study the living being instead of the environment, all objects in experience are looked upon as phenomenological; that is to say, they are all neutral with respect to their designations and functions in other orders of description. No object as it appears in experience is set into the physical order. Ten

views of this room as I regard it from ten positions are ten phenomenological objects, though the room is one. If I enter it a thousand times, experience numbers a thousand integral occurrences. Properties, parts, sequences and dependences are all set and determined by the biographical career of the living organism. So far as description goes there appear in the psychologies governed by this category, no parts or fragments of an abstracted physical order, no mathematical or æsthetical or ethical objects or orders, no formal space-time in which the setting of every object is determined by a fixed system of coördinates. Order there certainly is and a describable manifold; but these are determined by the biographical career of the organism and not by the principles of physics. The phenomenal world bears regular and assignable relations of a functional kind to objects of other orders; but these relations fall outside experience itself and demand hypotheses and explanatory assumptions. Thus a psychology of experience carries the implication that the active organism is more than an incident in the environment, which it views as belonging to the biological—not the psychological—order and as existing only by virtue of the biologist's complementary concepts of organism and *milieu*.

In the last forty years the category of experience has appeared in various forms, among which we may distinguish (1) the fluent consciousness described by James, (2) the conscious acts of Brentano, (3) the productive and creative powers of the Austrian School, (4) the constellated processes of Wundt, (5) attributive characterization, (6) functional performance of a psychosomatic kind and (7) the configured or structured experiences of the *Gestaltpsychologie*. As a modern instance of the psychology of experience I shall briefly consider the last-named, the *configurational* variety. This variety may, as I think, stand at the same level of discussion as the behavioristic stood under the category of objectivity and so serve our purposes of comparison.

We turn, then, to the form of psychology which sets its emphasis upon the structural wholes or unities of phenomenal experience. This is the doctrine of the *Gestalt*. No more

than in the last case did a new psychology appear without anticipation. It is a curious coincidence that both the configurationists and the behaviorists date their birthdays from the year of grace 1912.[8] Watson finds before that eventful year only the 'conventional' psychology of the soul,[9] exhibiting in his comparison of the 'old' and the 'new' as little accurate knowledge of his antecedents as he shows, in his neat derivation of the 'soul' from man's laziness, of the early history of mankind.[10] The exponents of the *Gestalt*, on the other hand, profess to believe that, prior to the same year of fate, only the crassest forms of naked associationism were to be found in psychology. There is a bit of comedy in the claim of the behaviorist that only the ghostly soul preceded his advent while the configurationist discovers, at the selfsame hour, a psychological desert in which only the dry and sterile mechanics of Hartley and the Mills is to be found. A plausible argument for Protagorean scepticism! If the behaviorist's depiction of all 'traditional' psychology as metaphysical maundering about the soul is a sheer caricature of the actual state of affairs prior to the Watsonian enlightenment in 1912, the configurationist's insistence that he found only arid associationism in the two forms of sensationalism and of stimulus-and-response is, at the least, an exaggeration. Associationism in plenty both before and since; but also much beside. There was the doctrine of the *Gestaltqualität*, traceable to Mach in the eighties, and notably to Ehrenfels twenty years before Wertheimer. There was Wundt's radical function displayed in *Trieb* or impulse, his unifying apperception and his rejection of the external bonds of the English for the fluid 'process.' There were the acts of Brentano, the synergies and fusions of Stumpf, the dynamic integrations of Lipps, and the fluent, unanalyzable stream of James.

The truth in the exaggeration is that psychology had

[8] Watson, J. B., 'Behaviorism,' 1924, 1; Koffka, K., PSYCHOL. BULL., 1922, 19, 531.

[9] 'Consciousness . . . is the keynote of all psychologies today except behaviorism,' Watson, J. B., *ibid.*, 3.

[10] Should we likewise refer to some primitive human frailty the lapse in historical research which led the scornful critic of the soul to place the opening of the first psychological laboratory in 1869?

tended to employ beyond its due the procedure of analysis. This tendency was fostered as much by the physiology of sense and the neurology of brain as by the external bonds of a mechanistic philosphy. Concentration upon the results of analysis into sensational elements tended to obscure the organic and integral character of the facts under scrutiny. Once disarticulated, structure had to be restored by associative recombination.

The theory of the *Gestalt* was fortunate in coming upon the scene when a well-formulated phenomenological view of experience was already established. Forsaking the uncertain concept of a 'consciousness' which was, in one form, a sum-total of processes and, in another, a series of dynamic and productive acts, many psychologists had already turned to a more empirical description of a neutral experience, to which the objects, the classifications and the performances of the physical world were alien. This experience was directly *given*, as we have seen, in concrete and unitary fashion, and its essential nature was therefore not to be derived by the artifice of an analytical process. Many psychologists made use of this empirical phenomenology, not waiting for the reproaches of the *Gestalt-theorie* to induce them to forsake the troublesome concept of consciousness. One primary service of the *Gestalt* was to suggest a more rigid criticism of the complementary concepts of analysis and association.[11] I have spoken of the fundamental concept of configuration as *unity*, and unity implies a logic of whole-and-part which contends for the primacy of the whole. If the whole is logically prior, then the parts become members and receive a *status* only when related to the unitary whole. In experience, so the doctrine contends, are to be found natural unities; and it is, first, the discovery and description of these unities, and secondly, the relating of them to like unities in the body and in the physical order that form the two main problems of the psychologist. It is natural, therefore, that the exponents of this doctrine should devote much time to the negation of the concepts of

[11] Wertheimer, M., *Zsch. f. Psychol.*, 1912, **61**, 161–265; 'Ueber Gestalt-theorie,' 1925; Koffka, K., PSYCHOL. BULL., 1922, **19**, 531; 'The growth of the mind,' 1924; and Helson, H., *Amer. J. Psychol.*, 1925, **36**, 342–370, 494–526.

elementary process, associative ligation and attentive control. It is wholly logical, moreover, that the psychologist of configuration should stand as squarely against the behavioristic substitution of movement for sensation, the fixed association of stimulus with movement, and the mechanical summation of reflex arcs as he stands against the mental analyst's elements, the one-to-one pairing of stimulus with sensation (*Konstanzhypothese*), and the associative construction of mental complexes. Had it been necessary to demonstrate to the behaviorist that the sole alternative to his view of the organism as a conduit leading from, and to, the environment was the ghostly soul (a sheer specter of his imaginational muscles and glands), the doctrine of configuration would have sufficed. It appears uncertain, however, whether the behaviorist will assimilate enough of the widely current conception of *experience* (by no means confined to the adherents of the *Gestalt-theorie*), given in phenomenological immediacy, to modify his fantastic contentions about consciousness and the soul. With its emphasis upon unitariness and cohesion, both in neural function and in experience, the configurationist naturally modifies the old stress upon stimulus. Stimulus, as he contends, is only one determinant of the output of the organism, whether that output be regarded as movement, as perception, thinking, or emotion, or as some other form of phenomenal experience.[12] The organism is itself a co-determiner; not merely a tangle of sluiceways conducting the world through a porous body. The immediate and essential condition of experience is a coherent and unitary function of the central nervous system itself. Stimulus may modify to a certain variable extent the structures of experience; but it never really accounts for them. This functional integrity of the body and of its systems is not, as you will readily agree, a doctrine peculiar to configurationism. It was Flourens' reply to Gall, Jennings' contention against Loeb, Wundt's argument for a supreme unifying apparatus in the brain; and it now appears to mark a decided trend among the students of

[12] Koffka, K., 'Psychologie' (in 'Die Philosophie in ihren Einzelgebieten'), 1923, 513.

neurology away from the conduit-and-conductor view of the old neurone theory.

Although the doctrine of *Gestalt* did not invent the conception of stimulus as the occasional cause for modifying the function of a unitary bodily system, we must give it credit for clearing up the logic of functional unity and also—far more important—for planning and carrying out scores of experimental researches which tend to establish its claims and to question the adequacy of any associational category whether behavioristic or psychological.[13]

It is important to remember in this review that the doctrine of configuration is not the only successful attempt to retain the territory earlier held by 'consciousness.' In its retreat from this region the rear-guard of behaviorism usually remembers only the scene of its defeats, while battling with a category of existence which it failed either to subdue or to bring under a productive subjugation. The category of experience phenomenologically regarded is no more reserved for the *Gestalt-theorie* than the category of objectivity is the peculiar property of the behaviorist. Always the advertised brand has the advantage of inducing the public to 'think of it' when the public thinks of the class which it is eager to represent. A great deal of the output of Continental, English and American laboratories bears the stamp of the phenomenological category. A survey of five of the representative periodicals in the field of normal, adult psychology for a recent year (1924 or 1925) nets 154 original articles, of which 98 (virtually two-thirds) are based upon the phenomenological concept of experience, and most of these articles are experimental.[14] Comparatively little of this factual material falls within configurationism. In the current psychologies of experience the concepts 'consciousness' and 'mind' are seldom used; and, when they are, they generally escape the platitudinous reproach brought against the hypothetical 'mentalist.' This may be fittingly said of the laboratory under whose

[13] See, *e.g.*, the recent volumes of *Psychologische Forschung*.

[14] The periodicals searched were *Arch. f. d. ges. Psychol.*, *Zsch. f. Psychol.*, *Psychol. Forsch.*, *Brit. J. Psychol.* and *Amer. J. Psychol.*

hospitality we meet tonight. Although no definitive exposition of the categorical setting within which research at Cornell proceeds, it is our fault if we do not share the fruits of its important and consistent labors. As I understand it, the main responsibility assumed by the current psychologists of experience is to give a faithful and coherent account, written from the standpoint of the living individual, of what actually goes on when men listen, confer, resolve, act, think, worry, fear, plan, understand and misunderstand, remember and forget, construct fantasies, fall into illusion, and generally fail or succeed in discovering a *modus vivendi* amongst their fellows and in the midst of nature. While they apprehend the possible advantage, in certain problems, of abstracting from the existence of all these matters, the experiential psychologists doubt whether a transcription in terms of the environment (with the organism offering but a polarized passage-way) or in terms of hypothetical powers and energies, suggested in the clinic, can take the place of description.

A word here on the bugaboo of 'introspection' may be condoned; although a candid reading of no more than two or three accessible statements [15] upon its general place in the psychology of experience would suffice to eliminate most of the current misunderstandings. As used at large the word has many meanings; now it is a contortionistic turning-inward to catch something immaterial and therefore vague and elusive; again, it is a private confession of the individual about his own self; once more (a turn which certain behaviorists like to give the word)[16] it is the verbal report, or a response upon a response. In view of the multitude of meanings the use of the word seems to me to be as impracticable as the use of 'consciousness.' I find both terms superfluous. So far as I know, however, no human psychology dispenses in practice with what we may call *report*, however violently certain theorists may declaim against 'introspection.' Let me point

[15] Especially Müller, G. E., *Zur Analyse der Gedächtnistätigkeit, etc.*, 1911, 1; Titchener, E. B., *Amer. J. Psychol.*, 1912, 23, 427, 485; Washburn, M. F., PSYCHOL. REV., 1922, 29, 84.

[16] Woodworth, R. S., 'Four Varieties of Behaviorism,' PSYCHOL. REV., 1924, 31, 259; Weiss, A. P., 'A Theoretical Basis of Human Behavior,' 1925, 71, 234*f*.

out the profitable use of report in the two types of psychology already discussed. The psychology of experience makes report a primary resource of description. In the first place, the observer *reports experiences themselves*. He may report likeness or difference, presence or absence. Under suitable instruction and controlled conditions those forms of report furnish practically all the material for the psychophysical metrical methods. Language is often convenient, but by no means necessary. A nod to indicate that the right-hand light is brighter or that the faint tone is heard is sufficient. That the word or the movement is of no importance save as it helps to record an experiential fact, significant for the problem in hand, is shown by employment of the same nod or the 'there,' 'yes' or 'absent' in a hundred different contexts. It is my general experience in the laboratory that the more report corresponds term for term with what the objectivist calls the 'stimulus,' the grosser and the less significant it is. The better the experimental control the simpler the report, no matter how wide the variability of 'stimulus.' Two visual movements may, for example, as in a recent experiment, be simply reported 'same' when one is conditioned by a stroboscopic sequence of resting lines and the other by a single line gradually displaced. The range of this direct report of experience is very extensive. That is attested by the wide use of it in recent research. That one psychologist may use it under the rubric of configuration, another as attributive designation, a third as an index of functional performance, and a fourth as raw material for statistical treatment has no fundamental significance.

The second use of the report is to indicate the *result* or *outcome* of an experience. This use is common in reciting nonsense syllables, in repeating tachistoscopic material, in reading experiments, and the like. Here the value of the report lies in the inference which the experimenter draws from the reported result of experience to the experience itself. Hundreds of laboratory problems move in this direction. The third main use of the report resides in the *comments* made by the observer upon the course of experimental events.

The whole significance of an experiment may lie in the observation that fixation changed, that a chance sound distracted, that the digits displayed were counted out on the fingers, that the head-rest was uncomfortable or the room stuffy. Here the value of the comments (which practically every experimenter uses) lies not in them as responses to parallel *stimuli* but as indicatory of important factors in the experimental setting.

The first use of the report corresponds to the direct noting of an object or event under scrutiny in zoölogy, astronomy or mineralogy. We may call it the *inspective* report; the second to the result observed upon heart-beat of cutting the vagus or the effect of pouring acid on marble; we may call it the *terminal* report: and the third is the common method of noting whatever *accessory* conditions bear upon the main problem in hand; I should call it the *commentary*. The uses made by the psychologist of experience include the character of the experience itself, its bodily antecedents, its relation to stimulus patterns at the receptors, its bearing upon the history of the psychological organism, and so on. The same psychologist uses the terminal report to recover phenomenal objects, to determine the limits of capacity, to ascertain the variability of function under varied instruction and varied stimulation, and the like,—always taking output as the index to other facts. The commentary he uses practically as all experimentalists use it in all sciences, as a check on variable errors. The objectivist also uses the report in this third form, of course; though he pretends to require only a 'response' of his subject. The terminal report he likewise employs at times, denying, however (though with a doubtful logic), that the output of the organism has anything to do with experience (or, as he prefers to put it, with 'consciousness'). The inspective report, the main reliance of the experientialist, is, of course, barred to objective methodology. The psychoanalyst, in the third place, may presumably use all three forms. His lack of empirical control, however, virtually replaces the commentary in its scientific form by a gossiping kind of patter from the subject, out of which the trained

practitioner sifts the important items. The terminal report also gives way to a rough clinical observation of the daily conduct of the patient. Whatever inspection there is, finally, in the 'confession' is so gross, so voluminous, and so shot through with human values, as to make the method incomparable with the one delineated. The psychoanalytic method is unique. I am not sure that we can do better than to reserve for it alone the title of 'introspection.'

Of the theory and the practice of psychoanalysis little need be said in this context. Exposition of the psychoanalytic arts of application to the private and social affairs of man, the discussion of cases, the contention of rival factions, speculative construction, denunciation and passionate defense, fill the records of the last three decades. What is left for comment? Our own interest lies, of course, only in the psychological significance of the principles and concepts of this doctrine. Though hailed as one of the 'new' psychologies, the locus of its influence chiefly lies beyond the borders of our own subject. Conceived by physicians and medical theorists in the shadow of the hospital, substantiated by the clinic rather than in the scientific laboratory, based upon the aberrant, the sensational and the abnormal in human life, it takes a direction widely divergent from the movements which we have just now examined. Its assumptions, its practices, and the currency of its doctrines bear testimony to the vivid and abiding interest which men sustain in their own wants and cravings and to the means adopted to attain, whether in health or in disease, the objects of their passionate desires. Against the individual detachment of other psychological approaches, psychoanalysis creates a social setting; against the grayness of intellect or of the business-like response to an insistent environment, this doctrine sets the high color of emotion, and in the place of orderly and progressive adjustment it depicts the unending struggles of a Laocoon. Is it, then, a complementary system to be united with others? Or is it essentially antithetical and rivalrous?

A basal category—if it could be found—might help us to decide. So complicated is the superstructure, however, so

many and loosely related the principles proposed and so varied in the hands of many are the doctrines themselves, that the descent to a simple logical foundation is not easy. The essential concept underlying psychoanalysis would seem, however, to be the concept of motive powers of mind by which the conduct of man is to be explained. This category appears in history with the faculties of Wolff, the competing ideas (*Vorstellungen*) of Herbart, the *idées forces* of Fouillée, the drives of more modern systems, and in many other forms. In the psychoanalyst's specific use of this category the mental powers appear in a theater of contention, struggle, indirection and illusion, wherein the characters are moved by mighty forces concealed beneath the stage or within their own unhappy breasts. The scene is essentially tragic, wherein the doctrine seeks to explain the dramatic movement and its significance by the sheer assumption of psychical forces. Censor, libido, ego-complex, conflict, and the like are hypothetical abstractions given attributes suitable for the explanation of observed facts, after the manner of physical agents and energies,[17] of gods, demons, and demiurges.

Many persons may agree with Thurstone's assertion[18] that the conduct of the psychoanalyst's 'person' is much more important than the reflexes, the habits, and the configurations of our prosy 'scientific' psychology. But important for whom and to what end? Desires, cravings, agonized delay in the satisfaction of wants are by their nature important. Their importance is imminent and undeniable. To identify human and social importance with importance-to-psychology is, however, a matter for disagreement if not for dispute. Possibly it is not worth arguing, for no one is to be convinced by the mere declaration that psychology is, or is not, identical with the dramatic study of human nature. But we may ask with more profit whether the return to hypothetical mental faculties and powers, which are by their nature withdrawn from the field of observation, is intended to improve the

[17] Lashley has recently contended that when the Freudian ventures to translate his principles into physiological terms, as in the 'free energy' of the libido, he runs counter to what we factually know of neural functions. PSYCHOL. REV., 1924, 31, 192*f*.

[18] Thurstone, L. L., PSYCHOL. REV., 1924, 31, 175*f*.

methodology of our subject. On the other hand, when we look upon the great number of ways in which psychoanalysis has afforded glimpses and insights into the manner of our functions and operations, of our motives and our scruples, we have to acknowledge that, in spite of fantastic guesses and verbal explanations, the doctrine has greatly enriched our psychological knowledge. It might be further said, as I think, that however violently we reject the terms and the concepts of psychoanalysis, psychology is not yet in a position to substitute an orderly and coherent set of facts and principles which shall be scientific instead of clinical and divinatory. Advancement in that direction certainly is toward, nearly every group and sect in psychology now making its contribution; and it appears that we shall continue with great profit to assimilate much of the wisdom of the psychoanalytical doctrines.

It is ambitious to expect a common logic to reduce to a common understanding all the various schools and parties to be found in any science. Our scientific predilections are largely determined by the influence of preferred teachers and treatises, by the easy use of familiar terms and concepts, by personal tastes, provincial and limited knowledge of the field, intellectual inertia, negative suggestion, the fashionable vogue of the moment, and practical benefit. What chance has logic and what claim upon us has a given fundamental category which we do not elect to like! Even the relative successes of our rival terms in the open field of research are hardly to be rated in an objective way. Neither the 'objective' behaviorist nor the matter-of-fact statistician of the foot-rule has thus far shown us a way to allow for our own prejudices or for the waywardness of our professional neighbors. We might ask how the particular virtues and uses of each major category appear in actual profit to the science; but there again valuation enters and each psychologist will desire to be his own measure of all things psychological.

If for no other reason than to exemplify the difficulty, however, I shall thus suggest a rough estimation of worth in terms of fulfillment. To this end I may first enumerate what

appear to be the main demands which we reasonably make upon any psychological system and then go on to indicate what seem to be the main accomplishments of each general type of psychology.

For the purposes of a comparative review, we may provisionally list the main problems of all psychologies in the following way. (1) The character and performances of the adult, human individual, (2) the developmental history of the individual and of the race, (3) the performances of other animals, (4) individual, class and racial differences among human beings, (5) deficiences and disorders of the human being, and (6) human socialization.

As for the first (the character and performances of the adult human being), the behavioristic category of reaction, in the form of movement, naturally stresses performance above character. There the main business of the organism is to polarize its environment into stimulus and response. The function of the organism is determined by the conducting systems, modifiable by way of simultaneous incitement (conditioning) and by way of temporal repetition (habit). Understanding and thinking would seem logically to drop out; although the conscientious behaviorist has taken great pains to translate these problems into his own vernacular. The effort has seemed to me at times to be comparable to the writing of thermodynamics in the Choctaw tongue or to the hazardous essays of the wire-walking artist to prove that all the essential human operations can be carried out under a Chinese umbrella in mid-air. Possibly the feat *can* be performed, but *should* it! The devotees of the vaudeville may applaud it and so may those who are entertained by grave and profound symposia devoted to the identification of thinking with incipient stirrings near the windpipe. Were I a psychoanalyst I fear that I should look upon the attempt as a wicked waste of sublimated libido. The metamorphosis of 'reasoning' into 'response' may suggest to the older generation at Cornell the late Professor Corson's simple formula for transmuting 'Middletown' into 'Moses';—"just leave off the 'iddletown' and tack on the 'oses.'" The transformation once accom-

plished to the advantage of the prophet, we may still have to anticipate forty years more or less in the wilderness of logical confusion. I like better the behaviorist when he damns—whether or not he comprehends—the psychology of the 'soul,' forsakes the old-fashioned problems of his masters, and goes his own way toward the understanding of the babe and of the beast.

Under the category of phenomenal experience the existential problems of performance naturally assume an important place. In *Gestalt-theorie*, for example, we find a long list of descriptive researches into perception which are, at least, in method and aim, as scientifically respectable as the nursery game of teaching the infant to converse at the advent of the milk-bottle, as the invention of a new alley-stop for the rat-maze, or as the thrilling quest of a 'liberty-reflex' in the distressed street dog clamped to the 'objectivist's' operating table. How far the concept of the unitary whole will go toward a final description of function and performance it is too early to say. It is ambitious, and I should commend, especially to the behaviorist, a close and understanding scrutiny of its advances. Here, as it seems to me, the concept of the *psychological organism* will be found useful. Once we realize that the biologist's organism is an abstraction, omitting aspects and functions which demand description and explanation just as much as digestion, movement and secretion do, we may then observe that the way to such a conception of the living being is barred by nothing more serious than a phobic hatred of the obsolete terms 'epiphenomenalism,' 'dualism,' 'closed physical series,' and the like. One ultimate advantage of such a conception is to lead us from the multitude of individual performances, in which somatic factors always play their part, toward a total and more or less unitary individual whose specific properties and capabilities give us exactly the view of character-at-large or 'personality' which is psychologically needed; and here we have much to learn from the deep soundings of the Freudians.

To the psychoanalyst who attacks our first group of problems, character stands preëminent over performance.

The key-word is 'personality,' and the primary tasks concern the underlying desires and cravings of the 'person' and the conflicts and inventions by which these desires and cravings are either frustrated or attained. Although this point of view tends to adulterate observation of the particular event by speculative constructions, there is to be set down to its credit the discovery that the biologist's conception of 'adjustment' obscures as much as it enlightens the plain facts of life. Conflict, disorder in functional systems, constant need of reorganization, futile thinking, ambiguous perception, false memories, misapprehension of motives and misunderstanding of oneself and of others, are much more fundamental to human life than is the objectivist's adjustive response designed to set right the individual and society and to justify the optimistic belief that 'all's right with the world.' There is no doubt that behaviorists have done their part in certain problems under this heading of performance and character of the organism,—especially those problems of action in which organic movement plays a prominent rôle; but we may ask, as Woodworth did two years ago, whether or not they have always added by virtue of their use of the category of 'reaction.' Possibly their behaviorism is, in large measure, the rationalization of a deep preference for the problems of physical movement and its antecedents. The psychoanalysts might welcome the chance to fish out of their 'unconscious' plausible reasons why human beings, as well as other animals, seek to ignore, if not to destroy, objects suspended beyond the reach of their own interests and aptitudes.

Upon the problems of development (2) I must not dwell. That behaviorism has there made notable contributions no one can doubt. Its tendency toward simple interpretations has helped to discredit the dismal theory of instinctive units, designed to endow the infantile organism and to determine its developmental course. This ambiguous doctrine, advanced by Gall, Darwin, Preyer and G. H. Schneider (among a host of psychologizing biologists), made orthodox by James, and carried into the distant lands of education, social theory and bionomics by Thorndike, McDougall, Karl Pearson and

others, has crystallized into the conception of that 'original nature' with which the ready-made individual is supposed to be launched upon his career. Among its many corollaries is the notion, still current in some quarters, of an innate 'intelligence' which is susceptible of exact measurement by the methods of 'test.' In combating this obscure doctrine behaviorists have certainly performed a notable service. That improvement is not to be wholly credited to the basal categories of behaviorism, however, is indicated both by the continued use of the instinct by certain adherents of that doctrine and by the fact that many of us, not attached to a psychology of stimulus-response, rejected the old concept of separate innate powers long before the birth and christening of behaviorism. Nevertheless credit must be ungrudgingly given both here and to those behaviorists who have insisted upon an empirical study of the child unbiased by the dogmas of education and the vagaries of 'child-study.' In its preoccupation with 'learning,' behaviorism has tended to a specious simplification by conjoining the thinning-out and smoothing-down processes of habituation with the external addition of 'conditioned' units. The true processes of development would seem rather to be those of constant reorganization and of rebuilding upon new plans and new specifications. This modified view is now being urged by certain behaviorists themselves, as well as by students of neurology. Such a change of view tends to weaken the main tenets of behaviorism, however, for it inserts between the stimulus-response poles of the environment, essential properties of the organism itself.

Configurationism too has, as we know, lately advanced its claims into genetic psychology, at once attacking the associationism implicit in the reflex, simon-pure and conditioned, and bringing to bear the weight of evidence from experiment with the anthropoid apes and the discriminations of other animals.[19] Although many claims are still unsettled, no one can now approach these developmental problems

[19] Koffka, K., 'The growth of the mind: an introduction to child-psychology,' (Ogden, R. M., tr.), 1924; Köhler, W., 'The mentality of apes' (Winter, E., tr.), 1925.

without an intelligent knowledge of both sides. Neither can one neglect the contributions offered by the psychoanalysts. However strongly we may set our beliefs against buried memories and infantile lusts, we must acknowledge that search in the distant past for antecedents of adolescent and mature traits is very much in order. It is, as I think, no more pernicious to conjure up the picturesque demons of Freud than to cover the nakedness of our ignorance with the blanket of 'past experience,' or with the vagary of pathways progressively channeled and widened by an assumed frequency of repetition.

As for (3), animal performance, no one can, as I think, compete with the behaviorist. That was the scene of his first empirical achievement and there his accomplishment has been notable. Whether the individual is content to limit his psychology of the animals to the immediate observation of performance, applying the category of reaction, will naturally depend upon his systematic outlook. As for myself I find the scientific use of inference as permissible in the comparative laboratory as in astrophysics, and the conception of the psychological organism as illuminating as in human studies. Neither are the claims of *Gestalt* to be overlooked, as in the detection of 'insight' among the higher vertebrates. We must remember that all the factual results of the objectivists are, in principle, just as usable by other psychologists, provided only they are obtained by sound scientific procedures. In this particular field the psychoanalysts are scarcely at home because, as we may suppose, social duties and restraints are well-nigh negligible among the fortunate non-human animals.

In the discrimination of kinds and classes (4), in 'Individual Psychology' as it is called, the earlier attempts at typifying, by way of imagery, reaction-times, and the like, were limited in their success. The search for a simple indicator of the person has usually miscarried; though it may be rewarded in the future. Various opinions prevail regarding the value for individual differences of such a quantitative indication as the I.Q. and related expressions. If the meaning

of such terms as 'intelligence' and 'character' is ever agreed upon by the quantifiers, if it is shown that these aspects of the individual are really measured by the proposed methods, and if these designations are found to represent the essential differences obtaining among human beings, then quantitative measures may form a sound basis for individual psychology. In the meantime, the apparent remoteness of such a goal should not blind us to the very great practical benefits to be derived by 'testing' for the maximal amount of accomplishment in a given direction which may reasonably be expected of a given human being. In its application to the affairs of education, industry and business, the test-methods may easily become attracted to a common-sense form of behavioristic reactionism.[20]

Some men find the psychoanalytic subdivisions of humankind (*e.g.*, into introverts and extraverts) very important; but no one who has not already made psychoanalysis a general confession of faith will, as I venture to believe, see, in such gross classes, a definitive psychology of individual likenesses and differences. To me some of them are immensely instructive, for they cut deep into human tissue; and, relieved of their speculative context and subjected to experimental control, they may greatly advance our knowledge.

Among the psychologists of experience, the champions of the *Gestalt* appear to be most directly interested in the recovery of the person and the designation of personal or individual traits. Stressing the primary unity of experience and looking at the individual as the highest expression of organic integrity, they may presently be in a position to define those essential differences of integration which distinguish individuals. Until now they have given only vague intimations of the principles of temporal integration throughout the life course; and here I think that Watson at least, among the behaviorists, has gone well beyond them. But Watson misses, of course, the whole conception of progressive changes

[20] Such social distinctions as Weiss proposes seem to me to be little more than labels having no intrinsic relation to the doctrine of behaviorism. (See 'A theoretical basis of human behavior,' 1925, 209*ff*, 395*ff*.)

in experience itself, only making the environment responsible for the changing reactions of the body. Here again the notion of the unitary *psychological* organism seems to promise aid in distinguishing those fundamental differences among human beings which include *all* the organic resources and aptitudes and which progressively manifest themselves throughout the course of life.

When we pass to our last two topics, (5) disorders, and (6) socialization, it becomes still more difficult to set comparative values upon our three basal categories and the psychological structures reared upon them. In the realm of mental disorder it is natural to give first place to psychoanalysis, which is at once a medical doctrine and a curative means. As a doctrine it seeks to understand the widely variant forms of mental disorder; as a medicament it seeks to recompose and to alleviate. Its chief procedure is clinical and its method there is diagnostic. It divines rather than observes, in the sense of the sciences. As an aid to divination it assumes that mind is of a certain character, governed by hypothetical principles of operation, and subject to the influence of assumed forces. The psychological part of psychoanalysis thus lies behind the medical doctrine and practice as a body of assumptions concerning the nature of the human mind. In comparing psychoanalysis with other forms of psychology, therefore, it is wise to abstract from its clinical and curative aspects as well as from the cult which it has engendered and which is espoused by devoted and zealous believers. As Freud long ago contended, the psychological background is always hypothetical and explanatory. The forces assumed are not to be observed. They are designed to rationalize the observations. We should judge them, then, either by their logic or by their relative adequacy in explaining observed phenomena. The logic is, no doubt, the logic of all forms of faculty, whether physical or mental. They are agents endowed with those powers which human experience suggests. Thus contrary ends of society and of the individual suggest conflict; forgetfulness and automatic actions suggest the unconscious, and so on. It is obvious that such assumptions

are makeshifts which are sanctioned only by our ignorance of real causes and conditions. Whenever we shall be able to replace these vague dynamic terms by a more substantial knowledge of the integrated organism, regarded in its totality and inclusive of its developmental history, we shall make a decided advancement in the psychology of the abnormal. In the meantime, the more empirical procedures of behaviorism and of the psychologies of experience promise more ultimate and substantial gain than the speculative methods of psychoanalysis, no matter how successful in practice the clinical methods and curative measures may be.

A similar comment might be made upon the problems of social psychology. There the successes of psychoanalysis are less marked; though the assumption of a socialized human being as the starting point of Freudian doctrines has undoubtedly served greatly to enlighten us upon man's nature. Since this socialization of the individual rests in experience and is not to be confused with the physical movements due to mere spatial propinquity, I see no hope for an adequate social description in terms of a consistent and legitimate behaviorism. Those descriptions already at hand achieve an apparent success either by a reluctant lapse into terms of 'consciousness' (as in Allport) or by such an illicit extension (*e.g.*, by Weiss) of 'stimulus' and 'response' as really to involve an appeal to experience or else to faculties. Again I should say that a coherent doctrine of the psychological organism would offer the most direct and the most empirical approach to the scientific problems of disorder and of socialization. Such a doctrine disposes once and for all of the troublesome concept of 'mental' disease and it likewise relieves behaviorism of the impossible task of describing our socialized experiences in the bodily terms of movement and secretion.

In the central problems of adult human psychology, then, all our various modes of conceiving the task have led on to fruitful, though divergent, results. The 'objective' mode has been notably successful in depicting human performance by way of organic movement, in so far as performance is con-

ditioned by environmental factors. Since this kind of performance and these environmental conditions are obvious factors in human relations and human accomplishments, this mode has suggested a direct and practical way toward an easy, if not a profound, study of the human side of business, industry, and the other practical arts. Its emphasis upon movement has also led it to devote more time than the other modes to certain bodily functions, and their modification under practice and habituation. Of course it must be added that these problems are the common property of *all* the principal modes and are accessible as well to the psychology of experience and to psychoanalysis as to behaviorism. In this same field, the second or experiential mode has made the individual, regarded biographically, the locus of his problems, —instead of the environment. It has thus brought into focus many phases of human life which the behaviorist has been able to treat only analogically. Since the description of experience, in its various phases, is the particular genius, so to say, of this second mode, it would seem to be unwise, if not illogical, for the objectivist, attempting quite a different task, to seek to transliterate into his own language the facts of perception, memory, thinking and the like. As we have seen, the *conceptual* mode of explanation, found, for example, in psychoanalysis, seeks character and its springs, more than performance. Its proper field of endeavor in adult human psychology would seem, then, to be the substitution for our ignorance in this direction of a conceptual account of assumed forces and agencies which may help us to understand the hidden causes of human action and its defect.

In the second (the developmental) problems of psychology, the objectivist has been limited by his fundamental category. Since experience is eliminated, he seeks only to show what progressive changes the stimulus-response relation has undergone in the life of the individual. Although the genetic accounts thus far offered have chiefly rested upon such accessory hypotheses as innate movement, neural association, habit, and the conditioned reflex, the contributions have been considerable. Here a sane comparison of many animal forms has

greatly benefited the behavioristic type of objective psychology. The nearest approach to an experiential psychology of development is to be attained under the doctrine of the *Gestalt*, and this attainment is at the moment not much more than a program and a handful of clever and significant experiments. So badly muddled with the educational and social histories of the child has experiential genesis been, that progress has been woefully slow. We seem, however, to be on the eve of rapid advance. This advance promises to rest in part also upon the genetic works of the psychoanalyst, which, taken in their original setting, are too fantastic and too one-sided to stand independently as a sound psychology of development.

As for animal psychology, I have already noted the enviable contributions made under the objectivists' category; and I have also indicated the uses which the experiential mode is in a position to make of all these factual results.

In the psychology of individual and class differences no party can boast of great accomplishments. Quantitative measures of unlike performance have already proved their great practical usefulness; but these measures have never been really psychologized. Advance will doubtless be made in all three main directions; and here I think that psychologists of all creeds and all parties will do well to pool their small earnings and to plan in some concerted way for the future.

The same kind of consolidation in the study of disorders and of human socialization would, as I believe, be highly profitable. No party or school has at present a tenable psychology of the abnormal; though, as I have intimated, the psychoanalytical point of view is more especially designed for this purpose. The objectivist has translated symptoms and causes into his own terms; but I fail to see that he has gone far—save where he has illicitly used 'experience'—in his treatment of the abnormal. All agree, as I think, that the disordered individual is *functionally* defective; and I cannot see how the observed functional defects can be either described or explained without recourse to the total or psychological organism. Again, as for a real *psychology* of the social side of man, no one can boast. 'Social planes,' 'social instincts,' 'social impulses and emotions,' 'social responses,'

and the like, are plain make-shifts. And so are those sociological schemes to which have been affixed convenient labels stolen from the psychology of the street and of the newspaper. As in the last two problems, so here also each mode will have to proceed to its own conquests, taking advantage of the conquests of the others and restricting its ambitions to the limitations of its own fundamental concepts and its own peculiar genius.

It is now common for psychologists to urge an eclectic view, a compromise which shall mix in a little of each brand. So, they have said, let us include consciousness in behavior and admit the censor and the conflict into our view of consciousness. That way, as it seems to me, nothing offers. There is no real or profitable issue from mixing a bit of north with a modicum of south, from the compounding of such unrelated qualities as goodness and mass, or from the confusion of Chinese and Saxon tongues. If our contention that current psychology rests upon at least three radically different foundations is tenable, let us get what we can from each and have done with the notion that any one of them can serve the ends of the other two, or that we can, at least at present, keep our heads clear or make sense for psychology by talking and thinking in a polyglot tongue artificially made from unrelated roots.

If we are serious about 'objectivity,' let us embrace it, and use it in (say) the form of stimulus-response, whenever it is useful. Again, if our interest in the biography of the person or the organism (as we may choose to regard the individual) takes us to experience, let us seek appropriate problems and methods and concordantly envisage our subject. And, finally, if we seek to understand and to rehabilitate human life by reference to active and dynamic principles of a conceptual kind, then by all means let us go the way of the psychoanalysts. When, by mischance, we try to do the inappropriate thing, as we take our chosen way, then we may confidently expect to go wrong in the end. There is, so far as I can see, nothing to be gained by the contention that anyone can beat the astronomer at his game by peering into the dark with a tallow candle. Precise concepts, like fine tools and

delicate instruments, have precise and appropriate uses. We do not always act upon our frequent assertion that fundamental notions and categories are our servants and agents to be fittingly employed and to be retained only where, and so long as, they are useful. We easily fall into the belief that they are our hereditary banners and ensigns which must be both defended with our lives and carried straight into the enemy's camp to attest a spectacular triumph.

Our underlying notions are then useful. They are useful as heuristic guides to research and to construction; but we must remember that they represent less an ultimate truth than our own individual preferences. More is to be gained than men commonly acknowledge by a sympathetic insight into alien points of view and into the possible utility of concepts which one has not learned how to use. The history of the sciences is eloquent upon the enormous wastefulness of prejudice. The progress of learning is, at its best, painfully slow and its methods shockingly uneconomical; but nothing would seem to retard it more than the arrogant and uncritical assumption of supreme virtues in any one point of view. More beneficial is it to discover the relative virtues of all serious proposals and the appropriate uses to be made of each of our concepts and each of our fundamental categories. The history of our Association, extending through a third of a century, suggests that among its greatest services to Psychology is the annual occasion which it offers to meet under a truce upon neutral ground and there to regard with an open but critical mind all that is brought together, under divergent points of view, from the whole extended field of research.

'Well, that's over for a year,' remarked Spinoza with a sigh and a yawn.

'Yes, thank heaven,' exclaimed Wattsavior. 'And it is not too late for a bit of jazz from my old home town to visceralize our behavior and to help us to forget.'

Then the local committee announced that the penalties exacted by Fortuna had been duly paid and that the guests were free to indulge themselves as they would. So all sought the air with thanksgiving and relief.

THEORIES OF INHIBITION

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PART I

Conjectures with respect to the specific neural condition of inhibition are almost as numerous as the investigators who have studied its phenomena. Most of them, however, fall under one or more of relatively few fundamental hypotheses which may be listed as follows:

(1) Specific inhibitory nerve centers. (Setschenow, Langendorf, Oddi, Wundt.) [1]
(2) Wave interference. (Cyon and Brunton.)
(3) Artificially stimulated anabolism or the anabolic phase of self-regulated metabolism. (Gaskell, Hering, Wundt, and Verworn.)
(4) Drainage of neural energy. (Alexander James, William James, and McDougall.)
(5) Refractory phase. (Verworn, Froelich, Lucas, and Forbes.)
(6) Chemical theories. (Depressing drugs, humors, gland secretions, fatigue toxins, etc.)

Some of these hypotheses seem on further consideration merely to emphasize different sides of the same fundamental process. For example: In the ordinary course of the self-regulated metabolism, refractory phase practically coincides with the restitution process in which the irritability of a tissue gradually returns after reaction. It would consequently seem to be a matter of comparative indifference whether one identified the inhibitory process with anabolic restitution of irritability or with a refractory phase depression of irritability subsequent to reaction. Both processes cover approximately

[1] In the interest of economy, bibliographic references will be given only in connection with detailed discussion of the several hypotheses.

the same period and are demonstrable by identical or analogous experimental methods. Refractory phase can be experimentally divorced from restitution only when the latter reaches a supernormal degree as in the rebound or the 'subsequent augmentation'[1] or when the self-regulation of metabolism fails, as in those cases when the tissue is deprived of oxygen, when the metabolic equilibrium remains permanently on a lower plane, or when in death of tissue the last refractory phase becomes permanent. If the refractory phase theory of inhibition is right, the anabolic or restitution theory cannot be entirely wrong, but it is doubtless expedient to emphasize that aspect of the total physiological conditions which is most directly accessible and most easily measured. Moreover, as will appear later, the anabolic process as such is never a cause of decreased, but of gradually increasing irritability.

Even the wave phase theory of inhibition, notwithstanding the highly improbable forms that it first took, has a certain relation to the refractory phase which makes it not entirely independent. In a slightly different formulation and with a different description of the kind of waves, it represents one aspect of the same fundamental metabolic process that is involved in refractory phase. One may properly speak of waves of excitation, depression, and superexcitability which follow one another in irritable tissue. Every formulation of the concept of the refractory phase implies a wavelike process. It is only another way of describing the facts to say that phases of excitation and phases of depression cannot occupy the same point at the same moment in irritable tissue. It would not, however, be permissible to speak of wave phases of depression coinciding with phases of excitation in inhibition, since during the phase of depression, excitation cannot occur. They cannot be regarded as neutralizing one another. In any neural system they exclude each other.

Four theories in the list stand out relatively clear and distinct,—namely, those of specific centers, drainage, refractory phase, and humors. Each of these hypotheses as to the nature of inhibition is supported by sufficient theoretical or experimental evidence to command attention.

[1] Forbes, *Quar. J. Exper. Physiol.*, 1912, 5, p. 176.

The Hypothesis of Specific Inhibitory Centers

The first hypothesis to account for inhibition was that there existed a specific inhibitory nerve center. This was the easiest and most plausible way to explain vagus action and the inhibitory effects of higher centers on the reflexes of the cord. The inhibitory center was located chiefly in the optic lobes by Setschenow[1] and Langendorf.[2] They based the hypothesis on stimulation and extirpation experiments. When Langendorf found that the croak reflex was eliminated by cutting the optic nerve, he assumed that the ocular excitation was merely incidental, a stimulus for the action of the central inhibitory organ. In spite of the experimental evidence, the optic lobes were soon found to be quite too small an organ to explain the physiological facts and the hypothesis was extended to include parts of the cerebrum.[3] In opposition to Setschenow and Langendorf, Herzen [4] held that the assumption of inhibitory centers failed to explain the experimental facts. He held that extirpation of any part of the nervous system produces reflex depression by the exhaustion that follows excessive stimulation. It also produces reflex augmentation by restricting the force of the stimulation to the residue of the nervous system.

On the basis of experiments on the croak and clasp reflexes of the frog,[5] Goltz held that any part of the nervous system which sends impulses to a reflex center acts as an inhibitor, probably through the development of a certain amount of exhaustion. Finally, it was found that some centers give rise to either excitation or inhibition according to their peripheral connections. Nerves whose function is commonly inhibitory lose this function when they are cut and regenerated into new peripheral endings.[6]

The hypothesis of special inhibitory centers fits the psycho-

[1] *Physiol. Stud. über die Hemmungsmechanismen*, Berlin, 1863.

[2] *Arch. f. Physiol.*, (Du Bois Reymond), 1877, p. 96.

[3] Ferrier, 'The Functions of the Brain,' Ld, 1876.

[4] Herzen, 'Expériences sur les centres moderateurs de l'action réflexe,' Turin, 1864.

[5] Goltz, 'Beiträge zur Lehre von den Functionen der Nervencentren des Frosches,' Berlin, 1869, pp. 41 *ff.*

[6] Langley, *J. Physiol.*, 27, p. 237.

logical facts quite as badly as the physiological. Since there is no mental phenomenon that may not operate to condition a decrement of some other, the inhibitory mechanism, whatever it is, must be as extensive as the neural conditions of consciousness. In the Wundtian schema the idea of a special inhibitory center is retained without encountering this difficulty by identifying it with an apperception center in the frontal lobes. According to Wundt's hypothesis, this center contains the "nodal points of conductions" whose abrogation produces disturbances that manifest themselves as impairment of intelligence. It operates as a link which connects each excitation that corresponds to an idea in the process of becoming clear with the current disposition of consciousness.[1] The center derives its function from the processes that go on there. Inhibition is not an exclusive peculiarity of the center. It is in the nature of an assimilation process and is found as a consequence of the production of work in all nervous tissues.[2]

The Hypothesis of Wave Interference

The wave interference theory of inhibition has its roots in Volkmann's [3] movement theory of nerve transmission, antagonism, and sympathy. In opposition to the theory of specific inhibitory centers of Setschenow and Langendorf, Cyon [4] developed a wave interference theory. This interference was not based on the differences of the various wave phases but on the relative differences in the angular incidence of nerve fibers. By this means he explained the apparent fact that under certain combinations of stimulation, inhibitory nerve fibers may give rise to positive excitations. Cyon held his theory of wave interference merely as a tentative hypothesis which he referred for confirmation to future histological research. The hypothesis aroused a storm of protest and remained practically unsupported. A wave phase interference theory which Cyon

[1] Wundt, 'Principles of Physiological Psychology,' translated by E. B. Titchener, p. 316.

[2] *Op. cit.*, p. 70.

[3] Magnus Handwörterbuch der Physiologie, 1844, 2, p. 539.

[4] *Bulletin de l'Académie de St. Pétersbourg*, 1872, 16, p. 114.

discarded as less probable was definitely held by Brunton,[1] who first suggested the rôle of distance and rate of transmission in determining interference. While commonly discredited, it has had certain echoes in recent physiology.[2]

The Anabolism Theory of Gaskell

The Anabolism Theory of Gaskell,[3] based on Hering's doctrine of the dual nature of the metabolic process, was given its first clear statement in 1884. Briefly stated, it assumed that in the dual metabolic process the anabolism phase may interrupt or inhibit the normal dissociative process following stimulation. Analogous hypotheses were elaborated by Wundt in the first edition of his 'Physiological Psychology' and by Verworn [4] under the concept of Biotonus. Gaskell obviously regarded his theory as a tentative working hypothesis for which enough evidence existed to warrant serious consideration and experimental test. His argument may be summarized as follows: The action of cardiac inhibition is specific. It is found only when the endings of the inhibiting fibers are intact and only in those cardiac tissues which are reached by the inhibitor. In opposition to the supposition of a specific inhibitory nerve action which operates on a passive muscle, Gaskell pointed out that the properties of muscular tissue are deeply affected by stimulation through the inhibitory action. That this inhibitory effect is not catabolic is evidenced negatively by a lack of all indications of exhaustion and positively by an increased rather than a decreased excitability after inhibition. These phenomena seemed to indicate anabolic action. Additional evidence was derived from the change in sign of the electric currents of action when the submaximary gland was acted on by inhibiting fibers. Gaskell expected definitive evidence from further investigations on the heart. These have never been forthcoming. The anabolic

[1] *Nature*, 1883, **27**, p. 419.

[2] Sherrington, *Quar. J. Exper. Physiol.*, 1913, **6**, p. 254.

[3] *J. Physiol.*, 1884, **7**, p. 1.

[4] 'Erregnung w. Lähmung' Vortrag, Frankfurt, 1896. *Allg. Physiol.*, Ed. 3, Jena, 1901, pp. 512 *ff.*

theory was later abandoned by Verworn [1] because there was no known stimulus to assimilation except exhaustion and the presence of food. There is no evidence that any stage of recovery decreases irritability. On the contrary, return of irritability after reaction shows itself to be a gradual process in which each stage of recovery shows a correspondingly increased, rather than a decreased, state of irritability. In the absence of dissociative stimuli, this process of recovery seems to run a regular course, reaching a climax in the hyper-excitability of the rebound, but it may be interrupted at any moment by adequate dissimilative excitation. The theoretical improbability of the assimilation theory was made clear by McDougall's searching criticism.[2]

The Drainage Theory of McDougall

The drainage theory of inhibition is commonly credited to William McDougall [3] in both physiological and psychological literature. This is probably just, since in McDougall's presentation the drainage theory was given its clearest and most complete scientific statement. As a matter of fact, however, the essentials of the theory go back to Herzen and Schiff,[4] to Setschenow,[5] to Alexander James,[6] and more explicitly to William James.[7] Another apparently independent expression of the drainage theory is found in the jet pump hypothesis of Max Meyer.[8] In the several presentations of William James, McDougall, and Meyer, there was clear recognition of the fact that inhibition plays an important rôle in mental life. This was the psychological occasion for the theory. Their common physiological motive was the difficulty involved in the hypothesis of a special inhibitory neural mechanism. The concentration or drainage theory came to be regarded as the more plausible alternative.

[1] *Allg. Physiol.*, Ed. 5, Jena, 1909, p. 604 *ff.*
[2] *Brain*, 1903, **26**, p. 156.
[3] *Brain*, 1903, **26**, p. 153. 'Principles of Physiological Psychology,' London, 1905.
[4] *Op. cit.*
[5] *Op. cit.*
[6] *Brain*, 1881–2, **4**, pp. 287 *ff.*
[7] 'Principles of Psychology,' 1896, pp. 579 *ff.*
[8] 'Fundamental Laws of Human Behavior,' Boston, 1911.

In addition to the argument by elimination, the probability of the drainage theory was increased by its remarkable correspondence with a large number of facts of inhibition and apparent inhibition.

McDougall explains the inhibition of reflexes by the action of superior systems as follows:[1]

". . . In the intact state of the nervous system, they (the reflex arcs) are but parts of more complicated mechanisms, and the energy liberated in sensory neurones therefore diffuses itself through these more complex systems instead of throwing into action the relatively simple and isolated mechanisms of the spinal level." Similarly,[2]

". . . Each system involving higher-level arcs, is an inhibitory center for every other; the activity of each system brings about as a collateral effect the inhibition of all others. . . . All the phenomena of light- and color-contrast seem to be due to such inhibitions, and the very intense contrast-effects produced by a smoothly-graded zone of transition between the contrasting areas, seem to be explicable by this hypothesis only." What was proposed as a speculative suggestion at the spinal level "becomes a well-founded hypothesis when we consider the processes of inhibition in the arcs of the second level. We can conceive of no other way in which the antagonistic relation between the activities of the multitudinous higher-level tracts can be brought about." The fact that any mental event may more or less completely inhibit all others in its own persistence in consciousness becomes the crowning proof of the theory. "In no other way can we conceive as a physical process the exertion by *B* of such guidance upon the direction of the discharge from *A* that the discharge finds its way to *B* no matter in what part of the brain *A* and *B* may be."[3] The physiological theory of the drainage hypothesis is a suppositious analogy between neural action and the flow of fluids through tubes, funnels, or paths. So, as McDougall puts it.[4] "We may liken the system of

[1] 'Physiological Psychology,' p. 38.

[2] *Op. cit.*, p. 103.

[3] *Op. cit.*, p. 134.

[4] *Op. cit.*, p. 132.

neurones to India rubber tubes." While this is a "rough mechanical illustration, . . . it may aid us to conceive how the passage of an impulse through any one system of higher-level paths may inhibit by drainage the passage through any other."

The experimental evidence on which McDougall based his drainage theory was derived chiefly from experiments in contrast and in bi-retinal rivalry. His most fundamental experiment was a modification of Fechner's paradox. McDougall found under certain conditions that when two fields unequal in size and in illumination are presented one to one eye, and one to the other, the smaller, less brightly illuminated field reinforces the other in that part which falls on the corresponding area of the other retina. That is to say, the bi-retinal bright images reinforce one another. In that part of the field where the image of the larger area is uni-retinal, it is darkened. That is to say, the light area that falls on a part of one retina which corresponds to a dark area of the other retina is darkened. In terms of drainage theory he interprets this as follows: "The loss of brightness of the monocular field is a necessary condition of the gain of brightness of the combined field." [1] It seems doubtful if this crucial experiment will stand analysis. The whole field of bilateral rivalry is too little known to permit its exploitation as proof of anything except our ignorance. If the drainage theory were established, the experiment could doubtless be brought to correspondence with it, but as an experimental proof of the theory it leaves much to be desired. Certainly other explanations of the McDougall phenomena are possible without evoking the hypothesis of drainage. One might logically hold that a summation of the central bi-retinal light areas could be paralleled by a summation of the more peripheral light and dark areas rather than caused by it.

The physiological conditions of the phenomena of contrast are almost equally obscure.

I believe that any argument for a supposed physiological fact which is based on the assumption that it is the only way to

[1] *Brain*, 1903, 26, p. 189 *f.*

account for a mental phenomenon must accept the burden of proof. As a working hypothesis, it needs the support of our knowledge of related events and of unambiguous experimental test.

The Quantum Theory of Neural Energy

Whatever the arguments in its favor, the drainage theory of James, McDougall, and Meyer meets several difficulties in the modern physiology of the nervous system which were not so apparent at the time of its formulation. All three assume a flow of nervous energy which McDougall christened 'neurin.'[1] Apparently this nervous energy was supposed to exist in a definite quantum that might be distributed into various channels and pass from point to point in each individual neurone and from one neurone to another. It is doubtful if any other picture of neural action has been as productive of misinterpretation of vertebrate neurology as the hydrodynamic analogy to the flow of liquid in tubes. In the neural physiology of vertebrates there is no evidence whatsoever of a neural flux corresponding to the flow of blood. Yet the discredited hypothesis lives on as a didactic figure. Even in the most favorable case of conduction in nerve fibers, analogy to the flow of liquid through tubes does not hold. Such an analogy implies decrement between the nervous action at the exit of the nerve fiber from the neurone cell and its termination in the end brushes. As a matter of fact decrement is not discoverable under normal circumstances either in the main axone or its collaterals. The progress of neural excitation along the axone without decrement corresponds neither to the passage of a fluid nor a simple electrical current. There is evidence on the contrary that axone conduction consists of an electrochemical process, in which the excitation at each point of the axone becomes the condition for the excitation of the neighboring points.

Just as there is no indication of a flow of nervous energy within the axone, so there is no evidence of a flow of nervous energy from neurone to neurone. It is becoming increasingly probable that each element of the nervous system and every

[1] *Brain, Op. cit.*, p. 171.

part of each element is subject to the all-or-nothing law. It is either stimulated to maximal reaction, or it does not respond at all. This has been proved for motor neurones and muscle fibers. It is probable for the sensory axones. That any link in the chain of neural action should be a mere transmitter of energy now seems to be improbable. It is disastrous for psychophysiological theory when a mere didactic analogy or a 'rough mechanical illustration' is allowed to determine our conception of the probable action of nerve centers.

The flow of energy from one part of the nervous system to another is as improbable as that the energy of light reaches the cortex. Such an assumption is obviously opposed to the physiological facts. As a matter of fact each element is stimulated to reaction by changes in its vital environment and its metabolic reactions in turn stimulate other elements. This is the presumptive course of events within the axone, between neurone and neurone, and between neurone and muscle. As measured by its electrical characteristics the energy of muscular contraction is vastly greater than the energy of nerve action and the energy of conduction in an axone bears no relation to the energy of peripheral stimulation. In fact the electrical energy of any one element in the chain of neural events bears no fixed relation to that of any other. Each element probably responds maximally according to its specific irritability and refractoriness and according to the 'all-or-nothing law,' each in its own manner with its own threshold, latency, refractory phase, and possibly its own rhythm.

Perhaps the most plausible support of a neural quantum of energy that can be stored up and utilized on occasion occurs in the rebound and 'subsequent augmentation' of a reflex after inhibition. In this matter Forbes' experiments on the effects of prolonged inhibition may be regarded as crucial.[1] The physiological assumption at present is against a neural quantum of energy which may be stored, augmented, or drained.

In my records[2] of the competition of visual pursuit and

[1] *Quar. J. Exper. Physiol.*, 1912, 5, pp. 182 *ff.*

[2] J. EXPER. PSYCHOL., 1923, 6, pp. 179 *ff.*

vestibular reflex for the final common path to the external eye muscles, there is clear evidence that the inhibited excitor does not store up neural energy in the human nervous system. When either competitor becomes dominant after conflict it emerges on the same level as though it had been operative throughout the interval of inhibited effect.

Drainage and the All-or-Nothing Law

In addition to the difficulties which the drainage theory meets in its assumption of a quantum of available neural energy, it seems unreconcilable both with the all-or-nothing law and with the law of axone conduction without decrement.

The condition of suppositious drainage must be some property either of the stimulating system, or of the stimulated system, or of junctional tissue. The first possibility would seem to imply that the energy of a neurone could be internally controlled and directed, that it could be concentrated in one collateral and expended in one direction. There is no known mechanism within an axone and its system of collaterals for such a differential apportionment of energy. On the contrary, if in accordance with the all-or-nothing law each part reacts to its fullest capacity at all times or not at all, and if according to the law of normal conduction without decrement, no point offers any obstruction, it seems highly improbable that energy can be either directed or concentrated at any point of the collateral system. In cases where decrement is actually produced in the axone by the action of drugs, the inhibition is produced by the drug and not by drainage. It is probable that if the area of decrement is once passed, the energy of reaction in the axone returns to its former intensity.[1] Under normal circumstances, axone reactions once started apparently pass to the end of each collateral with the same energy with which they leave the area of excitation. While the all-or-nothing law cannot be regarded as conclusively proved for sensory nerves, the probability is now so strong as to create a presumption against any opposing theory.

While it is improbable that drainage is a property of the

[1] Lucas, 'The Conduction of the Nervous Impulse,' London, 1917.

stimulator neurones, there remains the possibility that drainage might be effected by the condition of the receptor neurones or the surfaces of separation. This would imply some property of these tissues that would produce a concentration of stimulation or by which the energy of stimulation might be exhausted or siphoned off in some predetermined way.

It is thinkable that readiness of the receptor might operate to absorb the energy of the effector. The supposition receives no support from analogy with the peripheral receptors. The physical condition of light, for example, suffers concentration and decrement solely from the physical properties of the objects that are in its path. The whole sensorium may be bathed in potential stimuli while only a small area is tuned to respond and its responses depend on its metabolic readiness. The degree of response is determined by the irritability of the tuned receptors on which light impinges rather than on the light producing energy in the universe. There is no serious question of draining the surrounding sea of energy that impinges on the unresponsive areas of the sensorium.

It is possible, however, that there is some peculiarity of the receptor poles of central neurones that vitiates analogy with peripheral receptor systems. It is thinkable that suitably excited dendrites have the power of collecting and siphoning available energy from surrounding tissue, but this would be mere speculation for which at present there is not a shred of physiological evidence. On the contrary, if each part of the axone of the stimulating neurone reacts maximally according to the all-or-nothing law, it is obviously impossible for it to react supermaximally. But without some such assumption the receptor neurone can have no effect on its normal excitor.

The case for siphoning by the surface of separation appears to be about the same. There seems to be no way in which a synapse could increase the reaction of a stimulator neurone or effect any other change by which drainage could be accomplished.

If the phenomena of suppositious drainage are finally reduced to variations in the irritability of the receptor

neurones, then the psychophysiological problem will shift to a question of the nature of that receptor change and it may eventuate that such changes in receptor irritability are adequate to explain the entire process. Whether the drainage theory could be adapted to present-day conceptions of neural action or not, is a question I am not competent to answer. I conjecture, however, that some of the phenomena that have been grouped under the concept of drainage could be equally well explained by the differential incidence of reinforcing stimuli and the conditions that lead to their relative effectiveness and ineffectiveness.

Experimental Test of the Drainage Theory

Notwithstanding these theoretical difficulties, the question of drainage inhibition cannot be settled quite so simply. The real test of the truth of the hypothesis is not its congruence or lack of congruence with more or less probable physiological generalizations. The real test is its congruence with experimental facts. Putting the matter in its simplest form one may ask, Is it or is it not a fact that decrement of a reflex regularly occurs as a correlate of increased spread of neural excitation? Is it or is it not a fact that spread of excitation so as to involve the cortex or the initiation of a cortical reaction actually inhibits a related reflex? If either of these questions can be answered experimentally in the affirmative, then drainage would have to be regarded as a fact which would force a modification of any neurophysiological generalizations with which it seemed to conflict. These questions of fact seem capable of experimental answer.

We already have evidence in the work of Shepard and Fogelsonger [1] that drainage is not the only mechanism of inhibition. In their experiments on association there was no related pathway by which drainage could occur. The inhibitions of association which they found were consequently incompatible with an exclusive drainage theory. Moreover, in the initial paper of this series,[2] I reported a significant

[1] PSYCHOL. REV., 1913, **20**, p. 290.
[2] PSYCHOL. REV., 1926, **33**, pp. 7–8.

decrease in the amplitude of the knee-jerk which was produced by the complete relaxation and deadening of the thigh. Since this deadening involved not more cerebral action but less eventuating in something akin to sleep, the consequent inhibition cannot be regarded as the result of drainage. Similarly, the well known depressing effects of drowsiness and sleep on the reflexes cannot be due to drainage since it parallels depression of cortical activity.

These facts, however important they may be as indications of the existence of other methods of inhibition, are obviously not conclusive against the drainage hypothesis. They merely prove that drainage is not the exclusive mechanism of inhibition.

The previous question whether increased spread of stimulation and consequent cortical reaction will annihilate a reflex by drainage is more fundamental.

In view of their obvious importance, I tried to devise experimental answers to these questions of fact. If the inhibition of a reflex by drainage actually follows the spread of excitation in the cortex, then it ought to be possible to inhibit a reflex by voluntary reaction to the same stimulus that normally excites it. Translated into terms of the knee-jerk this might be expressed as follows: The blow on the patellar tendon, which is the normal knee-jerk stimulus, could be made the stimulus for a voluntary finger reaction which released a telegraphic key and broke a recording circuit. If the drainage theory is a fact then the knee-jerks which are accompanied by this voluntary reaction should be either totally inhibited or consistently diminished in extent as compared with those in which no voluntary reaction occurs.

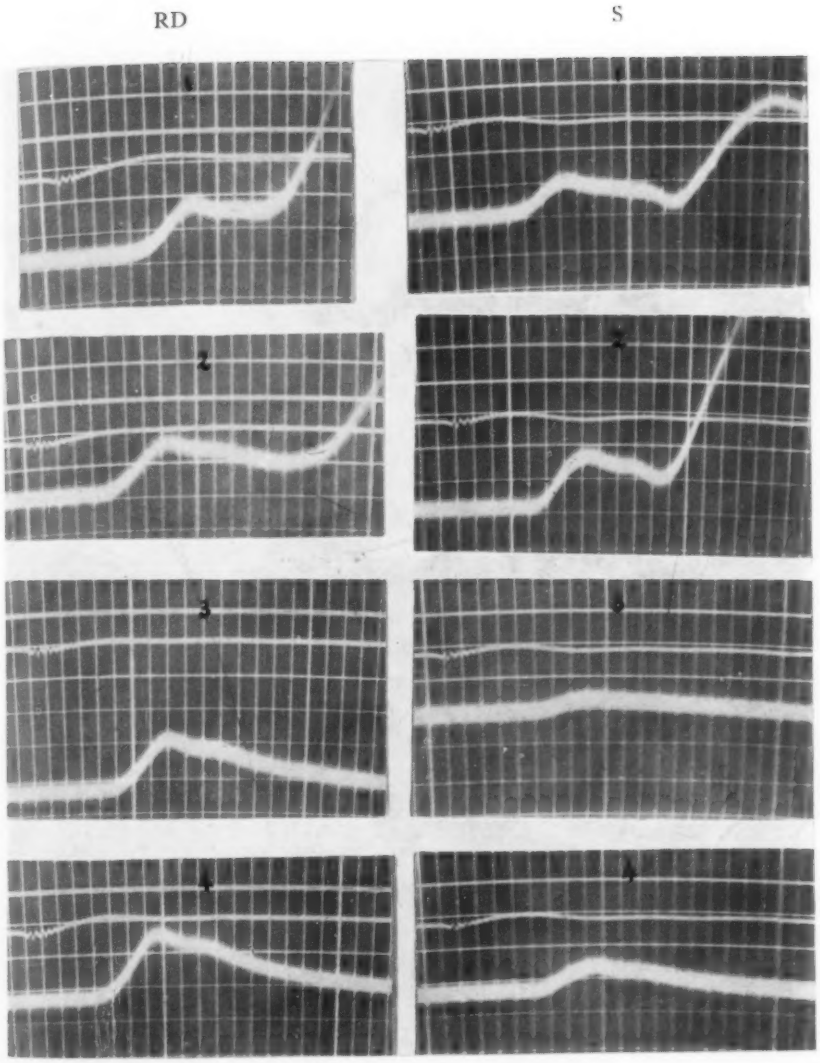
At first this test seemed to be a fair one. On further consideration, it was found to involve a number of difficulties. In the first place, the variability of the knee-jerk is notorious and although I have base lines extending over a number of years, moderate changes in the extent of the reflex would have to be regarded with considerable skepticism. Complete inhibition would have been a clear issue. That did not occur. Furthermore, the mere intent to react voluntarily might be

the occasion for a knee-jerk reinforcement which could cover up the depression due to drainage. This is probably not a valid objection to the experiment. Simple intent to react should not involve greater spread than intent not to react. The experimentally significant spread was provided for in the actual reaction.

In all cases the excitation which is started by the blow on the patellar tendon reached the cortex; that is to say, the blow was regularly perceived. This was true equally in the case of reaction and the case of no reaction. Increased spread of excitation in the case of reaction is evidenced by the experimental facts.

Finally, it is possible that the spread of excitation from the lumbar cord to produce finger movements involved such a distant neural pathway that drainage was consequently reduced or impossible. This possible objection should not be a real consideration according to the hypothesis. Change in pressure would theoretically occur throughout the entire hydrodynamic system, wherever one opened the stop-cock. However, as a concession to our ignorance concerning the whole matter and in the effort to observe every possible precaution, an analogous experiment was carried out with the lid reflex instead of the knee-jerk. In other words, we substituted a brain-stem reflex for a reflex of the lumbar cord. Moreover, to maintain a still closer anatomical connection between the reflex and voluntary systems, an identical final common path was utilized in both. That is to say, to insure the spread of excitation, the subjects were 'set' to wink arbitrarily whenever they heard the noise stimulus for the lid reflex. Since the latter experiment is open to less criticism than the former and since this is not primarily an experimental paper, detailed description of the knee-jerk experiments would probably be out of place. It suffices to report that the knee-jerk was never eliminated. There was, in fact, no characteristic or decisive difference between the reflexes without voluntary reaction and those in which voluntary reaction occurred. If any degree of inhibition was effected by drainage in that experiment, it was too slight to show in the relatively

PLATE I



few records that are available. The knee-jerk experiment, as it stands, failed to prove the existence of inhibition by the conceded spread of excitation. In this sense it constitutes experimental evidence against the drainage theory.

Similar results appeared in the lid reflex test of which eight characteristic records are reproduced in plate I.

These records were taken by means of a pendulum photochronograph which has never been described in print although it has been demonstrated on a number of occasions. This instrument consists essentially of a heavy counterbalanced pendulum, whose period can be modified through a wide range. The end of the pendulum carries a pack of sensitive photographic paper past a vertical rectangular slit in a screen. The slit holds a cylindrical lens of short focal distance, six centimeters long. This lens focuses a beam of light from a point source as a fine line on the sensitive paper. Time lines are introduced into the record by interrupting the recording light 100 times per second with a toothed disc which is mounted on the shaft of a synchronous motor. The motor is driven by electric connections in series with an electric tuning fork. The advantages of the pendulum chronograph in a psychological laboratory are its general reliability, its accuracy, the ease of counting the time lines, and the extraordinary quietness of its operation.

Our general technique for taking photographic records of the lid-reflex has already been described in various connections.[1] It may be called the eyelash shadow method of photographically registering lid movements.

The slightly curved lines of plate I are concentric abscissæ whose radii are determined by the length of the pendulum from its axis to the point of incidence of the recording beam of light. They are shadows of silk fibers which are stretched horizontally across the lens. The radial lines crossing the abscissæ at right angles are time ordinates 1/100 second apart. They were produced by interrupting the light in the manner previously described. The moment of stimulation is shown

[1] Dodge, R., *Zsch. f. allg. Physiol.*, 1910, 12, p. 41. Dodge, R., *Amer. J. Psychol.* 1913, 24, p. 1. Dodge and Benedict, 'Psychological Effects of Alcohol,' 1914, p. 56 *ff.*

by the sound vibrations in the fine line toward the top of each record. The lid reflex and the subsequent voluntary lid reaction where it occurs are shown by rising curves of the broad white line.

These eight records were taken in the order of numbering at the end of extended series after the reflex had settled down to relatively small variability. Subject R. D. was myself; subject S. was my assistant, Mr. Scofield.

The recording technique was adequate. The experimental test seems to be conclusive. Neither the patellar reflex nor the lid reflex is inhibited by a demonstrated spread of excitation. The drainage theory does not correspond to the facts.

The theoretical difficulties and the experimental test combine to render the drainage theory of inhibition highly improbable in human neural tissue. Unless new evidence for it is discovered, it ceases to be available as an explanatory principle in human behavior.

PRINCIPLES OF THE WORK DECREMENT

BY EDWARD S. ROBINSON

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Since Professor Thorndike first waged war upon Kraepelin's treatment of the curve of work, psychologists have been extraordinarily cautious in discussing this whole topic. But no matter what suspicions have been cast upon the existence, importance, or generality of *initial spurt*, *warming-up*, and other alleged features of the work curve, the body of experimental data, coupled with casual observation, leaves no serious doubt about the reality and generality of that decrement in efficiency which is produced by relatively continuous work. Professor Thorndike, it is true, is not greatly impressed by the magnitude of such decrements as are usually obtained. To quote him: "The most important fact about the curve of efficiency of a function under two hours or less continuous maximal exercise is that it is, when freed from daily eccentricities, so near a straight line and so near a horizontal line." [1] Still, it must be remembered that mental arithmetic and the other psychological functions employed by so many experimenters are, as Professor Dodge has insisted,[2] extremely complex. In fact they contain such a variety of part activities that their continuous exercise does not, after all, involve a large factor of continuity. Nevertheless, even in the case of these complex functions, a work decrement is almost always obtainable. We are thus entirely justified in placing this decrement from continuous work beside the practice increment as one of the basic facts of behavior.

Yet the mere knowledge that continuous work tends to produce a loss in efficiency in that same work is only moderately interesting. The scientific importance of this effect begins to appear in its true light only when one notices that

[1] Thorndike, E. L., 'Educational Psychology,' 3, p. 69.

[2] 'The Laws of Relative Fatigue,' PSYCHOL. REV., 1917, 24, pp. 89–113.

the magnitude of the work decrement is a matter of great variability. The low level of efficiency produced by prolonged work may suddenly disappear upon the occurrence of some change in the total stimulating situation. On the other hand, work which has remained at a high level of efficiency despite an apparently exacting continuity may suddenly show a marked and unexpected decrement. Here, too, the peculiar nature of the decrement may be due to some alteration in the total complex of stimuli, internal and external, which is affecting the worker. These are only samples of a variability which every psychologist has noted.

Curiously enough, though, the more variable the work decrement has been shown to be, the less interest there has been in its investigation. Instead of advancing upon this territory as soon as its richness was apparent, experimenters have, with few exceptions, increasingly neglected it. Critical writers have shown that the work decrement, in the vast majority of cases, reflects more complicated events than energy depletion, or cell and synaptic refraction. This has had the fortunate effect of putting an end to much dubious speculation. It has had other effects which are not so fortunate. Principally it has put much of the discussion of the work decrement on a plane of such cautious empiricism that the possibilities of establishing general principles have largely been ignored. Doctor Watson's chapter on 'The Organism at Work'[1] is a good example of this over-conservatism. There are numerous experimental results here, but little in the way of interpretation. The author warns his readers against neural fairy tales and against the gnarled issues of psychophysical parallelism. All this is wholesome enough in a negative way, but the only important positive interpretation in which he indulges is that which easily disposes of the greater fatigability of manual functions as compared with laryngeal in terms of the relative size of the muscle areas involved in the two cases. This general caution is not entirely mysterious. Psychologists have always had a way of putting entities into the background of observable phenomena and

[1] 'Psychology from the Standpoint of a Behaviorist,' pp. 348–385.

then imagining that experiments would reveal these entities in something like their absolute natures. Back of the work decrement something called 'fatigue' was once supposed to exist. It is only natural that, as soon as the supposedly simple nature of this 'fatigue' came under suspicion, confidence in the possibility of distinguishing the more from the less fundamental factors in the work decrement grew weak.

Nothing could be further from the truth than an assumption that the failure to discover a single, simple physiological cause of the work decrement establishes an equality among the facts related to that decrement. One can attempt an evaluation of the conditions of this occurrence without assuming that all decrements are reducible to the principles displayed in the reactions of the nerve-muscle preparation. Indeed, the very writers who have shown most vigorously the complexity of the factors underlying the decrement have, at the same time, come forward with positive suggestions. Professor Thorndike has made much of the distractions, such as headache and muscular strains, which the work itself produces. He has even suggested—and he has gone forward into experimentation with this suggestion [1]—that the decrement in complex processes is most intimately connected with the loss of what he calls 'satisfyingness.' Professor Thorndike's suppositions regarding the principles of the work decrement are, however, mainly suggestive. They are not very exhaustive and they are not very specific. And few efforts have been made to press into rigorous form what Professor Thorndike calls the 'biological theory of fatigue.'

Professor Dodge's name must be placed beside that of Professor Thorndike among the critics of single-factor interpretations of the work decrement. Although attacking the technique of certain of Professor Thorndike's own investigations, Professor Dodge has also shown a preference for biological rather than mechanical explanations of the effects of continuous work. But the most important point is that he,

[1] 'Fatigue in a Complex Function,' PSYCHOL. REV., 1914, 21, pp. 402–407, and 'The Curve of Work and The Curve of Satisfyingness,' *J. Appl. Psychol.*, 1917, 1, pp. 265–267.

for one, has tried to get his positive suggestions about the work decrement into something like precise form. The results are two 'laws of relative fatigue.'[1] These laws are not intended to cover all of the causes of the work decrement. One of these is 'true' or 'physiological' fatigue, which, following Verworn, Professor Dodge identifies with the relatively simple phenomenon of refractory phase. The 'laws of relative fatigue,' as I see them, state the conditions over and above refractory phase which are fundamental factors in the work decrement. The first of these laws is expressed as follows:

> "*Within physiological limits all fatigue decrement in the results of work is relative to the intensity of the stimulus.*"[2]

This might be called the 'law of stimulus intensity.' The second law, which might be called the 'law of competition,' is expressed as follows:

> "*In any complex of competing tendencies the relatively greater fatigue of one tendency will tend to eliminate it from the competition in favor of the less fatigued tendencies.*"[3]

There can be no real doubt about the contention that competition and stimulus intensity are important determiners of the work decrement. Nevertheless it is fair to wonder whether there are not other equally fundamental conditions of this phenomenon which can legitimately be formulated. It is fair, also, to wonder whether there might not be an advantage in stating the principles of the work decrement in more definite terms.

Taking the Dodge principles as a basis, I have tried for a more adequate systematization of the conditions of the decrement. The results are contained in seven main propositions which will be set forth in the present paper. The terminology in these formulations is, like Professor Dodge's, that of *stimulation* and *response*. There may be the objection that this terminology imposes unnecessary limitations upon the conception of work decrement, unless *stimulation* be carelessly taken to mean any antecedent psychological activity and

[1] Dodge, R., 'The Laws of Relative Fatigue,' PSYCHOL. REV., 1917, 24, pp. 89–113.
[2] *Ibid.*, p. 105.
[3] *Ibid.*, p. 102.

response any consequent psychological activity. While this difficulty may as well be admitted, at the present moment it is not of great importance. The situations which the work curve represents can be handled more readily in terms of the *stimulation-response* conception than in terms of any other conception of equal simplicity. There is another preliminary point the making of which may avoid misunderstandings. Professor Dodge has treated the principle of refractory phase as though it belonged upon a different level of fundamentality from that occupied by his laws of 'relative' fatigue. Because I am not at all certain that this distinction is justified, the formulations to be presented here are made in such a way as to leave the question open. Certainly we are not to understand that, whereas refraction has a chemistry, the facts represented by the laws of relative fatigue have not.

As a final preliminary point I wish to recognize that all work curve phenomena are to some degree dependent upon the criterion of efficiency under consideration. *Accuracy* decrements sometimes occur earlier or more markedly than *speed* decrements. *Satisfyingness* decrements sometimes have a large independence of decrements shown in other terms. Whether the seven principles which are about to be presented hold for measurements of satisfyingness, is not at all certain. In the present state of our knowledge this criterion is at best a rough and ready affair, unquestionably important, but not rigorously definable. Neither is there a clear applicability of these principles to such variations in cardiac activity, CO_2 production, general tonicity, and so on, as are associated with psychological work. I do feel quite sure, however, that these seven principles all hold for the decrement of either accuracy or speed. Of course there is no intent to imply that, because of this fact, the decrements in these two forms of efficiency are ever equal.

Now for the principles!

Principle 1: *The work decrement of a given S–R (stimulation-response) connection is relative to the recency of the previous functioning of that connection.* The isolation of this recency factor is perhaps most apparent under relatively simple con-

ditions of stimulation and response. Such an experiment as that of Professor Dodge on the refractory phase of the wink reflex [1] deals with the influence of recency of functioning upon the efficiency of an *S–R* connection. The results, the reader may remember, deny the absolute refractory phase of Zwaardemaker and Lans. As the time interval between stimulation and stimulation increases, there is a *gradual* increase in the probability and completeness of the second response. By experiments, departing further in the direction of physiology, this same graduation of the refractory phase has been demonstrated by Broca and Richet and by Lucas.[2] It is not only under such simple conditions that this recency factor is susceptible to study. The course of recuperation—for that is, after all, what these refraction studies trace—may quite as well be observed following a long series of operations of a given *S–R* connection. Actual experimentation along this line has so far been meager but the problem, itself, is clear. There is no reason why we should not presently give as much attention to the curve of recuperation as we have given in the past to the curve of work.

This recency factor may, of course, be thought of in even more general terms. The decrement from disuse (forgetting) is quite as closely related as is the work decrement to the time interval between stimulation and restimulation. The highest efficiency value usually lies at a medium recency. Early increases in the interval between stimulation and restimulation show an elimination of refractory phase and thus a rise in efficiency, while later increases in this interval favor habit disintegration. A possible exception is the case of reflex connections, where disuse may not always diminish efficiency, at least for long periods.

Principle 2: *The work decrement of a given S–R connection is relative to the frequency of the previous functioning of that connection.* Here again the fact is expressed that refractory phase is, or can be, a factor in the work decrement. But in this second principle the important additional point is made

[1] 'The Refractory Phase of the Protective Wink Reflex,' *Amer. J. Psychol.*, 1913, 24, pp. 1–7.

[2] Quoted by Verworn, 'Irritability,' pp. 157 and 158.

that refractory phase is accumulative. That is to say, the probability and completeness of response may be determined not only by the recency with which that particular *R* has been elicited, but also by the frequency with which it has been elicited. This double formulation of refractory phase can be justified on experimental grounds. If recency of previous response were the sole condition of refraction, we should have to rule out refractory phase as a possible factor in those decrements which occasionally occur in psychological work curves only after a given *S–R* connection has functioned continuously for some little time. Such a sudden collapse in efficiency as Painter [1] secured after several hours of arithmetical work points toward the independent importance of frequency as a condition of refraction. The occurrence of work curves possessing decelerations in decrement development also indicate the independent importance of frequency. There is, moreover, reason to believe that work may exhibit a refraction which is associated with recency of repetition without at the same time exhibiting a refraction associated with frequency of repetition. Assume that each operation of a certain *S–R* mechanism produces at once a marked refractory phase. The existence of this refractory phase means that there will be a certain delay before this mechanism can again operate. Now this delay following the single operation may be the best protection against any marked accumulation of refraction. Thus, simple refraction may to a degree prevent accumulative refraction, a fact which is likely to escape attention unless we discriminate between recency and frequency as conditions of the decrement.

Like recency, frequency of functioning is associated with the decrement through disuse as well as with the work decrement. If efficiency in general is under consideration, optimal work conditions involve some medium frequency value, since very high frequency values are associated with the work decrement and very low frequency values with disintegration.

Principle 3: *The work decrement of a given S–R connection*

[1] 'Efficiency in Mental Multiplication under Extreme Fatigue,' *J. Educ. Psychol.*, 1916, 7, pp. 25–30.

is relative to the connections between that S and other R's. While this principle is not completely independent of the first two, it is not to be deduced directly from them. The fact that one *R* connected with a given *S* may function in place of another *R*, or may block the functioning of that other *R*, is intimately related to the recency and frequency with which that other *R* has functioned; but since recency and frequency can condition incapacity without the presence of competition and since the results of recency and frequency are surely different when competition is present, the fact that competition of responses can alter the decrement requires separate formulation.

A question arises as to how the competing responses vary, because the importance of competition as a factor in decrement almost certainly does vary. It is fairly clear that these competing responses can vary in kind, in number, and in strength. The experimental isolation of these modes of variation and the measurement of their relative importance would, of course, be an important achievement, but the problem is much more difficult than most of the others toward which this paper points.

Principle 4: *The work decrement of a given S–R connection is relative to the strength of that specific connection.* The precise influence of competing responses upon the decrement is undoubtedly affected by the strength of the connection subject to competition. But that is only one reason why this fourth principle is important. While I know of no direct experimental study of the question, it is reasonable to assume that where only a single *S–R* connection is detectible or significant, the specific strength of that connection will influence the magnitude of such decrements as this connection may develop. Some form of the conditioned reflex method would probably be ideal for the experimental proof and elaboration of this principle.

Principle 5: *The work decrement of a given S–R connection is relative to the qualitative integrity of the S throughout the work period during which the decrement develops.* In the first two principles the assumption was made that we were dealing

with simple *S–R* connections or with connections whose complicating features were functionally insignificant. In the third, however, there is an important complexity in several possible *R*'s for a single *S*. In this fifth principle there is the further admission that the *S* is not, or at least need not be, identical throughout the period represented by the work curve. It is very generally acknowledged that, during what is usually called mental work, the work, itself, produces qualitative changes in the stimulational circumstances under which the work is performed, and that such changes may be fundamental causes of decrement. Although not to be deduced from the third and fourth principles, the detailed operation of the fifth may reveal the simultaneous and closely related operation of the former two. The alteration of the *S* which continuous work produces may affect the efficiency of action because:

(*a*) The new condition of stimulation may mean the addition of some competing responses and the elimination of others (Principle 3), or,

(*b*) The new condition of stimulation may mean a change in the strength of the connection between stimulation and the desired response (Principle 4).

Principle 6: *The work decrement of a given S–R connection is relative to the quantitative constancy of the S throughout the work period during which the decrement develops.* The continuous operation of an *S–R* connection may produce a decrement of a certain magnitude so long as that *S* remains at the same intensity. But the decrement may be diminished or augmented if the intensity of the *S* is altered. It is possible to conceive of such an alteration in intensity as simply a special case under the wider, qualitative, fifth principle. Nevertheless, there is an element of safety in the separate formulation of the qualitative and quantitative principles. The sixth principle can be related to the third and fourth in the same manner as was the fifth. (See (*a*) and (*b*) above.)

Principle 7: *The work decrement of a given S–R connection is relative to the decrements which have developed in other S–R*

connections. This principle expresses the established fact that there may be a transfer of decrement from one connection to an essentially different one. Concerning the detailed conditions which determine this transfer, little can be said. We do have, it is true, the theory that the transfer of 'fatigue' depends upon the presence of common elements in otherwise different functions. But this theory has a discouraging circularity, since the final test for the presence of common elements seems to hinge upon whether and to what extent there are transfers of decrement between them. Another theory of transfer, which is still taken seriously by some writers, holds that the difficulty of a first task rather than its qualitative nature is the basis for transfer of decrement from it to the performance of an immediately succeeding task. At first glance this theory would seem to be susceptible to easy experimental investigation. But the haziness of the conception 'difficult' renders the problem formidable. Perhaps supporters of this theory would, if pressed, be forced to admit that the only adequate criterion of 'difficulty,' in the sense in which they are using the term, is the amount of decrement transfer occurring from the function under consideration to other functions. In such an event this theory would become as tautological as the common-element theory. However, the principle of transfer of decrement in the general form in which I have stated it has undeniable validity, and more exact knowledge of its operation awaits only a sharper conception of the problem. As long as we study and argue about the effects of basket-ball practice upon multiplication, or of multiplication upon the memorizing of nonsense syllables, we shall have to be content with rough, qualitative findings which confirm the principle of transfer (which no longer needs confirming) but which add little to our systematic knowledge about it. We shall get little further with this matter of decrement transfer until we begin to work with functions differing from each other in relatively simple and definable ways.

In enumerating the above seven principles I have not attempted to bring them into point for point comparison with

the principles of Professor Dodge. Something of the kind can now be done. Principles 1 and 2 of this paper lie, as I have suggested, outside of the territory covered by the actual 'laws of relative fatigue' as Dodge has formulated them. These first two principles coincide rather with certain remarks about refractory phase which in the Dodge paper occur preliminary to the formulation of the conditions of fatigue relativity. Professor Dodge accepts Verworn's identification of fatigue (decrement coming from a series of operations of a connection) with simple refractory phase. I presume that, like Verworn, he feels more interested in this identification than he does in making distinctions between the simple and the accumulative effect. Since Professor Dodge is not, however, primarily concerned in his paper with refractory phase and its exhaustive treatment, there is no justification for making much of this issue so far as he is concerned. In regard to my formulations, it should be said that they do not for a moment disagree with anything Verworn has said about the fundamental identity of all refraction. I have simply taken the stand that the refraction resulting from the single operation of an *S–R* connection does not furnish grounds for inferring the result of a relatively continuous series of operations of the same mechanism. The third of the present principles is one of competition. Yet it is not the only principle required for understanding the place of competition in the determination of the work decrement. For a complete picture of how competition operates here, the fourth, fifth, and sixth principles are also needed. Professor Dodge has simply stated in one general law the importance of competition without going further with the analysis. Insofar as the fourth principle is independent of the competition factor, it covers ground untouched by 'the laws of relative fatigue.' Professor Dodge's 'law of stimulus intensity' is represented by the sixth principle of this paper. But, in the interests of precision, I have felt a necessity for formulating as a fifth principle a statement of the dependence of the decrement upon the qualitative integrity of the stimulus. Quite possibly Professor Dodge felt that this matter of qualitative integrity is clearly enough

implied by his 'law of stimulus intensity.' The transfer principle (Principle 7) falls outside of the territory covered by the 'laws of relative fatigue,' yet surely it is essential in an adequate formulation of the basic nature of the work decrement.

In conclusion, there are a few general points to be made regarding the principles presented in the present paper. Are all of these principles operative in all decrements undergone by psychological processes as a result of continuous work? Certainly there are many decrements the explanation of which can safely ignore one or more of these principles. In the initial stages of analysis, however, all of these principles should be thought of as possibly involved.

Psychologists have assumed for some time that the causes of the decrement as it is obtained in most of their experiments are exceedingly complex. The fact that at least seven basic factors in such decrements can be enumerated confirms this assumption. But the enumeration goes further and reduces the complexity to some kind of conceptual order. Perhaps the main advantage of this systematization is that it points some of the directions in which more detailed knowledge of the nature of the work decrement may most profitably be sought.

CRITICAL COMMENTS ON THE '*GESTALT-THEORIE*' [1]

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I. Summary Statement of *Gestalt*-Psychology

1. *Gestalt*-psychology is, in its negative aspect, one form of a very widespread contemporary protest against atomistic psychology— that is to say, against the conception of psychology as the science of associated aggregates of mental elements. Koffka summarizes in the following words the theory which the *Gestalt*-psychologists unite to decry. "All . . . consciousness consists," he says, from this discarded point of view, "of a finite number of real, separable (though not necessarily separate) elements, each element corresponding to a definite stimulus or to a special memory-residuum." [2] A mental unit, in accordance with this atomistic view, is "a bundle of such elements"; it is a "purely summative group of simultaneous elemental events each of which corresponds to a single stimulus and no one of which is essentially related to the other." ("*Elementarvorgänge neben einander . . . von denen keiner mit den übrigen etwas zu tun hat.*") [3] In lieu of this theory the *Gestalt*-psychologist urges his own doctrine that every experience is a *Gestalt*, a configuration, to which corresponds a physiological, and ultimately also a physical,

[1] This paper was read in substantially the present form before the departments of philosophy and of psychology of the University of California.

[2] Koffka, K., 'Perception: An Introduction to the *Gestalt-Theorie*,' Psychol. Bull., 1922, **19**, p. 533[2]. To this paper, and to a still later one by Koffka ('Introspection and the Method of Psychology,' *Brit. J. Psychol.* (Gen. Sect.), 1924, **15**, pp. 149–161), the English-speaking reader is referred for admirable exposition of the *Gestalt-Theorie*. A more complete treatment, also by Koffka, under the more general title 'Psychologie,' will be found in 'Die Philosophie in ihren Einzelgebieten,' a division of the *Lehrbuch der Philosophie*, edited by Max Dessoir, 1923, pp. 495–603. For a detailed exposition, see also the first of four articles on 'The Psychology of *Gestalt*,' by H. Helson in the *Amer. J. Psychol.*, 1925, **36**, pp. 342–370.

[3] Köhler, W., 'Die physischen Gestalten,' p. 18. *Cf.* pp. 197*ff*.

Gestalt; and he substitutes for the conception of 'reproduced' experiences as due to the mechanical 'association' of one mental process, or idea, by another, his conception of the *Gestaltdisposition*, the tendency to reinstate in imagination a configuration already apprehended.[1]

By 'configuration' or 'structure' (for both terms are used to render the German *Gestalt*) [2] is meant a unity which is an "undivided, articulated whole," which can neither be made up by adding together separable, independent elements nor reduced to such elementary constituents. The classic example of mental configuration is the perception of melody; and a character alike of melodies and of many physical configurations is that they are transposable (*transponierbar*), that is, that "they remain intact in their specific qualities when the absolute data (*Gegebenheiten*) on which they rest are displaced in a definite fashion." [3]

a. By way of illustration Koffka describes, in terms of the *Gestalt-Theorie*, the experience of observing two squares of grey cardboard, side by side on a black background, and judging, let us say, the right-hand square to be brighter than the other. Under these circumstances, he says, one sees "the darker . . . and the brighter . . . not as separate and independent pieces of color, but as steps . . . ascending." By 'steps,' Koffka continues, he means not only "two different levels but the rise itself . . . which is not a separate, flighty, transitional sensation, but a central property of this whole undivided experience." [4] In similar fashion, he describes the consciousness of the equal length of two parallel lines as the awareness of a figure "to which the two lines belong as essential parts," [5] and he sharply contrasts this

[1] *Cf.* Koffka, K., 'Psychologie,' in Dessoir's *Lehrbuch*, pp. 537*f*, 540, 571*f*.

[2] Helson tells us that the term 'configuration,' less ambiguous than 'structure,' was proposed by Titchener. In the PSYCHOL. BULL. paper, already cited, Koffka uniformly translates *Gestalt* by 'structure'; in his book, 'The Growth of the Mind,' by 'configuration.' This paper ordinarily follows the latter usage.

[3] Köhler, W., 'Die physischen Gestalten,' p. 17. Köhler is here expounding the '*von Ehrenfels criterion*' of *Gestalt*—a doctrine from which, however, the *Gestalt-Psychologie* diverges. He adds accordingly that 'there might occur' cases of *Gestalt* where this criterion is not applicable.

[4] Koffka, K., PSYCHOL. BULL., 1922, **19**, pp. 540*f*.

[5] Koffka, K., 'Psychologie,' in Dessoir's *Lehrbuch*, p. 532.

awareness of configuration with the experience of the mere external connection, the plus-relation, or *Und-verbindung*, of the two lines. To quote him directly: "The lines are equally long. This means, I now see no longer one line and still another line but a unified (*eine einheitliche*) figure to which the two lines belong. . . .Out of the apprehension of one plus one has grown the apprehension of . . . 'symmetrical' pair. . . .In a word, out of an and-relation has grown a unified configuration."

b. In their more detailed treatment of configurations, *Gestalt*-psychologists have not yet arrived at the stage of systematic classification. "Configurations become," as Helson observes, "more articulate or more finely structured, simpler in form, more precise or less definite, poorer in form or chaotic." [1] Among these simpler configurations is apparently included that of 'ground' as contrasted with 'figure.' "Ground," says Koffka, following Rubin, "is more simply structured (*gestaltet*), more unordered (*chaotischer*)" than 'figure' which is "more penetrating, stable, and thing-like." [2] And, at the other extreme, configurationists—though they deprecate any sharp line of cleavage between perception and thought—none the less contrast perceptual with thought-configurations. Yet, as Koffka expressly admits, it still remains a problem for *Gestalt*-psychologists to "work out the fundamental types alike of perceptual and of thought-*Gestalten*." [3]

c. It must be noted that *Gestalt*-psychologists scrupulously distinguish this doctrine of the *Gestalt*, or unified 'whole,' from that of the *Gestaltqualität* or 'feeling of relation.' The two theories agree in their opposition to crass sensationalism, the doctrine that all experiences reduce to sensational elements; they differ in that the upholders of *Gestaltqualitäten* tend to regard or describe them as elements coördinate with, though different from, sensational elements; whereas the main point of *Gestalt-Psychologie* is to enforce the significance

[1] Helson, H., *Amer. J. Psychol.*, 1925, **36**, pp. 369[2].

[2] Koffka, K., 'Psychologie,' in Dessoir's *Lehrbuch*, pp. 556*f*. *Cf.* 'Perception: An Introduction to the *Gestalt-Theorie*,' PSYCHOL. BULL., 1922, **19**, pp. 557*ff*.

[3] *Ibid.*, p. 574[1] and *supra*.

of the conception of the whole as contrasted alike with that of the element and with that of the mere composite of elements. Now it clearly is one thing to say: This experience includes elemental 'feelings' of connectedness, direction, or similarity along with sensational elements of redness, softness, and the like; and quite another thing (the configurationist insists) to say: This is the experience of a connected, similar, articulated whole. Koffka accordingly expressly warns the reader "not to confound the old term of *Gestaltqualität* with the term *Gestalt* as it is employed in the new theory." [1]

d. From this elucidation of the concept of psychological *Gestalt* it is time to turn to a little closer scrutiny of the parallel physiological and physical configurations. The physiological correlate of the perceptual or thought-configuration, according to the *Gestalt*-theory, is no single neurone or sum of neurones but a whole physiological region (*Gebiet*), "for example, the entire optical sector from the retina to the cortex as a unified system" including "even the regions of the nuclei (*der Kerne*) in which the nerves that innervate the eye-muscles have their origin." [2]

The conception of the physical *Gestalt* has been chiefly the concern of Köhler. Helson admirably summarizes his theory in the following passage: "Although summative groupings of matter may be found in nature, a true physical system is more than a sum of parts. The classical example," he continues, "is found in a static charge distribution. If an electric current is conducted to an isolated wire, its total charges quickly come to a state of equilibrium and the whole surface of the conductor has the same potential. This static grouping remains the same so long as no other disturbing factors are present. Such a state of equilibrium Köhler calls the charge distribution. This distribution is not a mere sum of part charges since electricity cannot be taken away from any part of the system without influencing every other;

[1] Koffka, K., PSYCHOL. BULL., 1922, 19, p. 536[1]. *Cf.* also 'Psychologie' in Dessoir's *Lehrbuch*, pp. 521*ff.*

[2] Koffka, K., 'Psychologie,' *op. cit.*, p. 565[2]. (*Cf.* p. 593[1].) For an excellent summary of Köhler's treatment of physiological configuration, *cf.* Helson, H., *Amer. J. Psychol.*, 1925, 36, pp. 366*f.*

when this is done, a new state of equilibrium is effected, which represents the new charge distribution." [1]

ii. A significant feature of *Gestalt-Psychologie*, at least in the form which Koffka gives to it, has not yet been named. This is its conception of what may be called the configuring individual. For *Gestalt-Psychologie* is concerned not only with experiences, that is, with percepts and judgments and the rest, as articulate wholes, but is concerned also with the 'experiencing individual,' [2] with the conscious organism in reaction on its configured environment. When we adopt the integrative and functional 'attitude,' we then, Koffka says,[3] "begin with the organism in its environment and study all its reactions. Among these we may reckon also those which are usually termed consciousness, I mean the fact that the physical world exists for the organism as its own surroundings . . . and the other fact that the organism also knows itself, feels hunger and pain, rage or lust." [4] This abstractly stated conception of psychology as a study of the organism in conscious relation to the articulated wholes of its environment finds abundant illustration in Köhler's studies of animal behavior and in Koffka's account of 'The Growth of the Mind.' Both writers treat not of 'solid unalterable' mental contents but of responsive individuals.

The great defect of Helson's exposition of *Gestalt-Psychologie* is his neglect, or rather his denial, of the configurationists' implication of the experiencing individual. He says unequivocally that 'Mind,' for the *Gestalt*-psychologist, "becomes a phenomenon within a larger system of phenomena and amenable to precisely the same laws, with no peculiar properties."

[1] Helson, H., *op. cit.*, p. 365[2]. *Cf.* Koffka, K., 'The Growth of the Mind,' p. 118[2]; PSYCHOL. BULL., 1922, 19, pp. 551[3]*f*.

[2] Koffka, K., 'The Growth of the Mind,' p. 16[4].

[3] Koffka, K., 'Introspection and the Method of Psychology,' *Brit. J. Psychol.*, 1924, 15, p. 153[4].

[4] *Cf.* Koffka, K., 'Psychologie,' in Dessoir's *Lehrbuch*, p. 568[1], where Koffka criticises theories which do not conceive 'the animal itself' as concerned in learning; and pp. 571*f*, in which he describes a case of association as a situation in which "I dominate the activity, . . . the transition from one word to its opposite."

iii. The preceding paragraph suggests a comparison of configurational psychology with behaviorism. For, like behaviorism, *Gestalt-Psychologie* opposes the atomistic practice of breaking up unitary psychic realities into separate bits; and like behaviorism it deals with organic individuals reacting to the structural environment. But *Gestalt*-psychology diverges sharply from the extreme form of behaviorism in that it insists on an introspective, as well as on an 'objective,' procedure. Koffka lays special stress on this crucial difference between *Gestalt* doctrine and behaviorism. The behaviorist's reduction of psychology to 'mere mechanics of limb and physiology of muscle and gland' he characterizes as a *reductio ad absurdum;*[1] and he opposes to the behavioristic position the fact that "there is a consciousness, reports of which can only be made by the experiencing individual. Science," he adds, "cannot refuse to evaluate factual material of any sort."[2]

II. Arguments on which the *Gestalt*–Psychology is Based

Contemporary psychological *Gestalt*-doctrine is supported by its upholders through two distinguishable though allied lines of argument: first, by the experimental study of certain human experiences, mainly perceptual, which must be conceived (so the configurationists hold) as articulated wholes, or structures, and not as aggregates of sensation; second, by studies of the behavior of animals indicating the continuity and unitary character of many animal reactions to environment. The pages which follow summarize, though in bare outline, arguments of both types.

i. The earliest of the experimental studies is that of Wertheimer.[3] He investigated the illusion of movement due to

[1] Koffka, K., 'The Growth of the Mind,' Chap. I., p. 20^{2}.

[2] *Ibid.*, p. 16^{4}. *Cf.* 'Introspection and the Method of Psychology,' *Brit. J. Psychol.*, 1924, **15**, pp. 152, 160^{2}; a review of Watson's 'Psychology from the Standpoint of a Behaviorist,' *Psychol. Forsch.*, 1922, **2**, pp. 382–384; Psychol. Bull., 1922, **19**, pp. 537^{1}, 560^{4}; 'Psychologie,' in Dessoir's *Lehrbuch*, pp. 502*ff.*

[3] Wertheimer, M., 'Experimentelle Studien über das Sehen von Bewegung,' *Zsch. f. Psychol.*, 1912, **61**, pp. 161–265.

the successive presentation of unmoving figures. He found (1) that if two lines, one horizontal, the other oblique, are successively exposed, each for about 200 σ, there is no illusion of movement and the lines are seen one after the other as separate and stationary; (2) that if the lines are very swiftly exposed (each for about 30 σ) one after the other, they form a single unmoving figure, $\angle$; (3) that if the successive exposure times lie between these two extremes either line may be seen to move, *e.g.*, from the oblique position to the horizontal or *vice versa;* (4) that at the optimal exposure time, the appearance is that of a single moving object which moves back and forth and turns on itself, whereas (5) with certain other exposure times [1] each of the two lines appears to move independently. No one of these illusions can be explained as a complex of sensations due to eye movement, for the illusions persist during fixation.[2] Nor can these illusions be explained, Wertheimer argues, as an inference following on the assumption that we suppose ourselves to be perceiving a single object and consequently infer that the successive appearance of its parts *must* be due to its motion. For, quite apart from the observers' introspective denial of the occurrence of any such reasoning, this inference-hypothesis is contradicted by the fact, already stated, that in some cases the two lines seem to move separately, not as parts of a single figure, which means that the illusion of movement sometimes occurs without an accompanying illusion of the identical single figure.[3] Wertheimer concludes, accordingly, that this visual illusion of movement must be described neither as mere sensation-composite nor yet as inference but as immediate unitary perception; and that its physiological correlate must be conceived as a distinctive unitary process (*Gesamtprocess*), a circuit-effect (*Umkreiswirkung*), not as a sum of separate, single excitations.[4]

A similar study has been made, more recently, by E. Lindemann, of the apparent expanding and contracting

[1] *Op. cit.*, p. 186.
[2] *Op. cit.*, p. 182[4].
[3] *Op. cit.*, pp. 187*ff.*
[4] *Op. cit.*, pp. 250[3], 252[1] with Note 3. *Cf.* p. 248[1].

movements of suddenly illuminated visual figures (circles, ellipses, triangles and the like).[1] He especially stresses two features of this phenomenon: first, that one and the same suddenly illuminated figure may assume distinctly varied forms; and second, that these apparently changing figures show the definite tendency toward a simple form (*Tendenz zu guten und einfachen Gestalt*). These characters are evidently irreconcilable with the theory that these illusory percepts are mere complexes of sensation corresponding, each for each, with a definite retinal stimulus, for obviously the retinal stimuli remain the same whatever the appearance of the given figure. It follows, Lindemann argues, that these observed expanding and contracting figures are conditioned, not by the excitation of independent retinal elements, one retinal element to each part of the figure, but rather by what he calls a displacement-process with direction (*gerichteten Verschiebungsvorganges*) in the excited regions, a process which takes place as a whole (*im Sinne des Ganzen*). "The correlate of this process is," he continues, "experienced as a perception of movement."[2]

A third experimental study, that of Köhler, on the comparison of successively presented tones, is more important for the bearing of the introspections of its subjects, than for that of its experimental outcome, on the *Gestalt-Theorie*.[3] There was no case whatever of comparison between present impression and memory image, viewed after the traditional fashion as independent and distinct phenomena. In fact, a memory image was not present in every case of comparison; and the actual experience in which the judgment consisted was not at all of separate tones but was rather an experience

[1] *Psychol. Forsch.*, 1922, 2, pp. 6–60.

[2] *Op. cit.*, p. 58.

[3] 'Zur Theorie des Sukzessivvergleichs und der Zeitfehler,' *Psychol. Forsch.*, 1923, 4, pp. 115*ff*. Working with tone intensities Köhler confirms and amplifies earlier results of Borak, Lehmann, Angell and Stratton showing that the judgments 'Greater,' 'Less,' and 'Equal,' as applied to pairs of successive stimuli, are regularly affected by the order in which the stimuli are given. In the case of his subjects, the ascending series were more often correctly given than the descending series (in the proportion of 35 to 19); and where the stimuli were equal they were judged 'greater' in 43 cases, 'equal' in 24, and 'less' in only 12.

of 'ascending' or 'descending' (*Aufspringen* or *Abfallen*) or, in a word, of 'progressing' (*Wachsen*).[1]

A fourth line of argument is derived from the study of the puzzle-picture experience. Examples are the perception of 'Rubin's goblet' which may also be seen as the profiles, back to back, of two faces, and that of the tablecloth border which may be apprehended either as a row of black T-shaped figures on a white ground or as a conventionalized, white, vine-leaf pattern on a black ground. Such a puzzle-picture percept, the configurationist points out, cannot possibly be conceived as a mere complex of independent sensations for the reason that the single sensations in the case of each of the alternating percepts (in our examples, that of black T or white vine and that of goblet or profiles) are precisely the same while *as wholes* the two percepts are utterly different.

This brief account of experimental results, interpreted by introspection, which are adduced in support of *Gestalt-Psychologie* must be supplemented by a reference to the work of its upholders in the field of animal behavior. In the earliest of these investigations Köhler trained chimpanzees, hens, and one three-year-old child to react to the lighter, *b*, of two greys *c* and *b*. The hens, for example, learned to peck at a *b*-tinted surface, the chimpanzees and the child to grasp at a grey-fronted box of shade *b*. Köhler then placed before animal or child a grey of shade *b* and another grey, *a*, as much lighter than *b* as *b* was lighter than *c*. In the majority of cases [2] the subject (animal or child) reacted not to *b*, the grey to which it had learned to respond, but to *a*, 'the lighter of the

[1] It is much to be regretted that Köhler does not give us actual excerpts from the introspective reports of his subjects.

[2] For details concerning apparatus, *cf.* Köhler, W., 'Aus der Anthropoidenstation auf Teneriffa, II. Optische Untersuchungen am Schimpanzen und am Haushuhn,' *Einzelausgabe aus den Abhandl. d. Königl. Preuss. Akad. d. Wissensch.*, Phys.-Math. Klasse, 1915, pp. 45*ff*, 56*ff*. For summary of results and account of procedure, *cf. ibid.*, IV., 'Nachweis einfacher Strukturfunktionen beim Schimpanzen und beim Haushuhn,' *Abhandl. d. Königl. Preuss. Akad. d. Wissensch.*, Phys.-Math. Klasse, 1918, pp. 18*ff*, 42*ff*, 46*ff*. The child invariably responded in the configurational way; the hen (Huhn VI.) in 77 of 100 trials; Chica, the chimpanzee, in 37 of 40 experiments. *Cf.*, for description of these experiments, Koffka, K., 'The Growth of the Mind,' pp. 139*ff*.

two greys': in other words, it made a configurational response. For, obviously, the significant experience of hen, of chimpanzee, and of child was not a specific sensation of grey but rather the apprehension of a certain relation between the terms of a unified series of increasing lightnesses—the perception, in a word, of a *Gestalt.*

The study, just reported, forms a part of Köhler's extended observation of the behavior of chimpanzees. For about four years, 1913–1917, at the Anthropoid Station, in Tenerife, of the Prussian Academy of Science, he sought to ascertain the degree of relationship between anthropoid apes and man especially as regards their 'apparently intelligent actions.'[1] His ingenious procedure was to observe his subjects in varied situations, such that in each a given goal could be attained only in some indirect fashion. The specific problem was to discover *if* and *how* the apes learned this indirect method. To illustrate: in one experiment on apes, dogs, hens, and on one child each subject was brought to a railing through which he could see food on the ground but could not get it. To attain the food it was necessary to face full about and to run round four sides of a fenced-off two-meter square.[2] In a more difficult experiment on the chimpanzees, the food was hung in a basket from the roof. The basket could not be reached at all from the floor, but if it were swung by the experimenter, its longest swing would bring it close to a scaffolding from which a chimpanzee could reach it.[3] Still other tests involved the use of implements. For example, fruit was placed so that it could be reached only by the use of a long stick; and later the stick could be gained only by leaping on a box.[4] Or a cluster of bananas could be retrieved only if they were first pushed backward, around a low fence, and then pulled forward toward the animal.[5] Or, finally, the successful attainment of the fruit required that it be pushed, by a stick inserted through the bars of a cage,

[1] Köhler, W., 'The Mentality of Apes,' transl. by Ella Winter, pp. 1*ff.*
[2] *Op. cit.*, pp. 12*ff.*
[3] *Op. cit.*, p. 19[1].
[4] *Op. cit.*, p. 183[2].
[5] Köhler, W., 'The Mentality of Apes,' pp. 239*f.*

toward a gap in the opposite wall of the cage. Through the gap, the chimpanzee, if he should drop the stick and run halfway round the cage, could seize the bananas.[1]

The most impressive general outcome of these experiments is the fact that all these indirect ways of gaining food, involving as they do the avoidance of obstacles and the employment of tools, were learned by some of the chimpanzees; and that most of them were learned by all the animals. More important to us is the fact that observation disclosed two distinct types of chimpanzee behavior in the face of these 'problems.' In the first, the 'random movement' type, the animal runs hither and yonder, perhaps tears madly about, and 'pulls and jerks wildly' at bars or stick or box. He is often unsuccessful but occasionally successful in attaining his goal.[2] In the second type of behavior, sometimes but by no means always preceded by movements of the random type, the animal surveys the situation and proceeds by a 'single continuous course to the attainment of its goal.'[3] So, in the experiment in which the basket of fruit, unattainable from the floor, is swung by the experimenter from the ceiling, "as soon as the basket is swinging, Chica, Grande and Tercere are let in upon the scene. . . . Chica quietly surveys the situation, suddenly runs towards the scaffolding, waits with outstretched arms for the basket and catches it."[4] Again, in one of the experiments requiring the use of a stick to secure food, "from the moment that [Sultan's] eyes fall upon the long stick, his performance forms one consecutive whole, without hiatus."[5] Köhler calls this type of behavior 'genuine achievement' and contrasts it with chance success. "It takes place," he says, "as a single continuous occurrence, a unity," whereas a successful chance solution consists of

[1] *Op. cit.*, pp. 263[2]*ff.*

[2] In the opinion of *Gestalt*-psychologists, Thorndike's experiments on animals are defective simply in that they were so arranged as to elicit only behavior of this first type. *Cf.* Koffka, K., 'The Growth of the Mind,' pp. 153*ff.*

[3] Quoted from Koffka's admirable summary of Köhler's experiments, 'The Growth of the Mind,' pp. 179*ff.*

[4] Köhler, W., 'The Mentality of Apes,' p. 19[1].

[5] Köhler, W., 'The Mentality of Apes,' p. 180[2].

"an agglomeration of separate movements which start, finish, start again, remain independent of one another in direction and speed, and only in a geometrical summation start at the starting-point and finish at the objective. The experiments on hens," he observes, "illustrate the contrast in a particularly striking way, when the same animal . . . first flies about uncertainly in zig-zag movements" and then "if one of these parts of the route leads to a favorable place, suddenly rushes along . . . in one single, unbroken run, . . . the one type of behavior succeeding the other so abruptly that no one could mistake the difference in the two kinds of movement." [1]

When the 'continuous' behavior follows on movements of the random type it is signalized by a noteworthy change of attitudes preceded usually by a pause. The animal "stops, then turns completely round, etc., the child looks about, suddenly its face lights up." [2] These constant instances of continuous, or unified, behavior, many of them without any preceding period of random movement, argue decisively, as Köhler points out, against the view that animal behavior is invariably of the random, 'trial-and-error' type and that animal learning is due solely to the repetition of fortuitous random responses. There may, indeed, occur learning of this random type, but it is, as has appeared, sharply distinguished from Köhler's 'true solution' or 'genuine achievement.' On the other hand, this movement "by a single continuous curve from [the] original position to the attainment of the goal" [3] is, in the view of *Gestalt*-psychologists, an obvious case of *Gestalt*-reaction, a response of the animal to the total situation.

Not only the continuous direction of the movement of response and the impressive pause at the outset of it mark this form of behavior as belonging to the *Gestalt* type. In still another fashion it is distinguished from chance, or random, behavior by the 'spontaneous variation,' namely, of the manner of its reaction to the environment. In innumerable

[1] Köhler, W., 'The Mentality of Apes,' p. 17.
[2] *Ibid.*
[3] Koffka, K., 'The Growth of the Mind,' p. 181[2].

cases the animal, so far from repeating, in mechanical fashion, movements which have previously been successful, attains his end by very different behavior. The animal for example, in the test first described, turns his back on the fruit which he wishes to obtain (a manifestly different reaction from that by which he normally seizes his food); and, in the last two tests, he must push the fruit away from him to attain it in the end. Again, instead of using invariably the tool that has proved successful, the chimpanzee often uses, in its place, any conceivable object—rag, rope, wire, blanket—for attaining food. This variation, in sensory quality of implements used and in character of bodily reaction, is entirely incompatible with the mechanical account of 'apparently intelligent' animal behavior as habitual repetition of chance behavior: in the opinion of Köhler and his fellows, it requires us to suppose that the animal apprehends and reacts to the situation as a whole; and this brings animal behavior—with animal consciousness, in so far as it occurs—under the *Gestalt*-conception.[1]

III. Critical Comments upon *Gestalt*-Psychology

The summary, presented in the preceding pages, of the central doctrines of *Gestalt*-psychology and of the considerations on which these are based, has been undertaken as foundation for certain criticisms of the system. These criticisms are, however, urged from the standpoint of complete agreement with the pivotal doctrines, positive and negative, of *Gestalt-Theorie*. All of them presuppose this fundamental agreement.

i. To begin with a relatively unessential matter: Configurationists often overlook the fact that the *Gestalt*-conception, important as it is, yet is not the only principle of psychological description. In other words, an experience is incompletely, however correctly, described as a configuration, or *Gestalt;* and not all experiences are effectively distinguished

[1] *Gestalt*-psychologists, at least as interpreted by Koffka, do not claim that it can be proved "whether consciousness is actually present or not in animal behavior." *Cf.* 'The Growth of the Mind,' pp. 15 *et al.*

from each other by the manner or degree of their integration. Emotion and volition, for example, do not differ merely or mainly as configurations either from each other or from perception and thought. As a matter of fact, *Gestalt*-psychology as a constructive doctrine is chiefly concerned with perception and learning, and with allied problems of thinking, judgment, and comparison. The result has been that configurationists, even in their more systematic writings, have touched very lightly on certain topics of psychology. Koffka, for example, in the essay which he contributes to Dessoir's *Philosophie in ihren Einzelgebieten* devotes barely fourteen of one hundred odd pages to 'Instinkte,' 'Affekte' and 'Willenshandlungen.' In 'The Growth of the Mind' he nowhere treats, in other than an incidental way, the teeming emotional life of children, the rise and decadence and modification of such basal experiences as liking and aversion, shame and self-display; and he virtually ignores also the child's volitional experiences, his constantly changing projects and ambitions. For though Koffka carefully describes experiments in choice-training [1] he makes no attempt to discriminate, in terms of *Gestalt*, between the cognitive and the volitional aspect of the choice reaction.

It is worth noting that configurationists themselves often tacitly admit the need of other descriptive categories than that of *Gestalt*. Koffka's admirable account of play is a case in point. He describes 'the world of play' as a 'world apart' into which we step 'out of the usual realities of life,' a world sharply contrasted with the 'single world-all in which everything must fit with everything.' [2] All this, as well as the more ordinary observation that in 'much of the play of children . . . inanimate objects are treated like living human beings,' [3] is good psychology but does not owe its distinguishing value to the *Gestalt*-concept.

ii. A more fundamental criticism of *Gestalt*-theory concerns its frequent confusion of analysis with atomism, that is

[1] *Op. cit.*, pp. 216, 232, 273, 283*ff*.
[2] Koffka, K., 'The Growth of the Mind,' Chap. VI., pp. 344[3], 345[2]*f*, 353[3].
[3] *Op. cit.*, p. 338[2].

to say, its characteristic assumption that an analytic procedure must inevitably result in the pseudodiscovery of psychic atoms. The configurationists rightly hold that, not only in our logical thinking, but originally and naïvely we apprehend wholes and not mere composites of separate elements; but they tend to ignore the fact that in our reflective moments we may also analyze these wholes into distinguishable aspects or elements and the further fact that such analysis is a *bona fide* part, though only a part, of scientific, psychological procedure.

Theoretically, to be sure, *Gestalt*-psychologists cheerfully agree with this view that 'the analyzed datum as well as the original one must be considered . . . '[1] though, as they properly insist, it 'is by no means the only or even a preferential method.'[2] But in practice they constantly contrast 'the analytic and substantial' with the 'integrative and fuctional' attitude[3] in such wise as to imply that analysis is the exclusive tool of the atomists. So Koffka says expressly of his 'larger wholes' that "they are not open to analytic observation," adding that "if an observer is to grasp their nature he must adopt a very different attitude, such as the attitude of . . . the wise old doctor [or] the clever diplomatist."[4] By these words Koffka obviously and quite unnecessarily plays into the hands of those critics who frankly identify *Gestalt-Theorie* with the pre-scientific point of view of the 'plain man.' Koffka should say, not that his 'larger wholes' are incapable of being analyzed but rather that they are not open to analysis of the atomistic type into independently existing psychic bits. For the quarrel of configurationists is not with analytic procedure as such, but with the 'bundle-hypothesis'; and they have no occasion to decry elements when these are conceived as organically related parts or characters of a psychic whole.

[1] Koffka, K., 'Introspection and the Method of Psychology,' *Brit. J. Psychol.*, 1924, 15, p. 154.

[2] *Ibid.*, p. 160[4].

[3] *Ibid.*, p. 153[4]. *Cf.* 'Psychologie,' in Dessoir's *Lehrbuch*, pp. 510*f* and *passim*, for Koffka's critical account of atomism under the heading 'Das Zerlegungs-Princip.'

[4] Koffka, K., *Brit. J. Psychol.*, 1924, 15, p. 161[1].

iii. A radically different defect in the *Gestalt-Psychologie* is its neglect of the concept, central to its own doctrine, of the experiencing individual, the conscious organism, which both knows itself and is also related, not only through muscular contraction but through consciousness, with its environment.[1] Even Koffka, though he so unequivocally asserts the existence of the experiencing individual, gives himself over for the most part to the consideration of the experiences (or experiencings) which are really, on his own showing, part-processes of the experiencing individual. He curiously overlooks the fact that this conscious organism is itself an articulated and integrated whole, that (in the words of Stern) it is "an existent which, despite the plurality of its parts, constitutes a distinctive . . . unity."[2] As such it of course possesses central properties[3] and its 'own laws of being.'[4] Assuredly, the *Gestalt-Psychologie* should have undertaken a study of these properties and laws.[5]

For still another reason *Gestalt*-psychologists ought not to ignore the doctrine of the conscious organism; it is essential to the conception which they propose as substitute for associationism. Atomistic psychology has always supplemented its account of images and percepts as mechanical composites of elements by the conception of percepts (or images) as exciting, 'calling up,' images corresponding with past experience. Hence configurationists have not only to replace the concept of the mechanically compounded idea, or content, by that of the integrated structure, or configuration, but they have also to put forward, in place of the concept of the separate ideas 'associating' each other the concept, ready to their hand, of the consciously unifying, integrating individual. In truth,

[1] *Cf.* p. 139, above.

[2] Stern, W., 'Die Menschliche Persönlichkeit,' pp. 4*ff*. *Cf.* Dr. G. Wartensleben, 'Die Christliche Persönlichkeit im Idealbild,' p. 1; "Das Wort *Persönlichkeit* . . . bedeutet psychologisch aufgefasst nichts mehr und nichts weniger als '*Gestalt*.'" (Both by Stern and by Gräfin Wartensleben, 'Person' is used in the sense of Koffka's 'conscious organism.')

[3] Koffka, K., PSYCHOL. BULL., 1922, 19, p. 541[2].

[4] Koffka, K., *Brit. J. Psychol.*, 1924, 15, p. 157[2].

[5] For such a study, *cf.* Stern, W., 'Die Psychologie und der Personalismus,' and 'Die Menschliche Persönlichkeit.'

the conception of the *Gestaltdisposition*, the tendency to integrate, which Koffka, in the most systematic of his writings, opposes to that of association,[1] of course definitely implies an individual which 'has' dispositions or tendencies.[2] In the following statement, already quoted, Koffka unequivocally acknowledges such an experiencing individual. He says, "If when a word is pronounced I can name its opposite," this does not depend on a determining tendency proceeding from the word "but rather on the fact that I dominate the activity, the functioning, the transition from one word to its opposite."[3] But, despite unambiguous assertions such as these, too often even Koffka relapses into what is very like atomism on a higher level, as for example when he describes comparison as a tension between two members of an integrated percept.[4]

The only explanation of this curious neglect by *Gestalt*-psychologists of a doctrine evidently essential to their system, implied or stated by them all,[5] is their common dread of the intellectualistic and partly atomistic doctrine that configuration is the result of an intellectual, synthesizing process imposed by a supernatural sort of ego on originally separate sensations.[6] An ego of this type would indeed be *persona ingrata* to all *Gestalt*-psychologists. The 'conscious' organism, however (whose existence configurationists always imply and, in their moments of clearest thinking, acknowledge), should on no account be confused with this rejected, bloodless, 'intellectual' ego.

[1] Koffka, K., 'Psychologie,' in Dessoir's *Lehrbuch*, pp. 537 *et al.* In 'The Growth of the Mind' (Chap. 3, pp. 123*f*) Koffka refers to 'inherited dispositions'; and in the PSYCHOL. BULL. article (1922, **19**, pp. 547*ff*) he describes an attitude as 'a readiness' of the organism for 'certain modes of response.'

[2] *Cf.* Stern, W., 'Die Menschliche Persönlichkeit,' Chap. 2, p. 70[1], for the conception of dispositions as forms of personal effectiveness, "Teilstrahlen der persönlichen Ursachlichkeit."

[3] 'Psychologie,' in Dessoir's *Lehrbuch*, p. 571[1].

[4] PSYCHOL. BULL., 1922, **19**, p. 546[1].

[5] *Cf.* in addition to the passages quoted from Koffka, and to the references above, Köhler's implicit approval of von Ehrenfels's distinction between one individual and many in the formulation of the criterion of *Gestalt:* Köhler, W., 'Die physischen Gestalten,' Sect. 24, pp. 35*f*. For a directer acknowledgment of the self, *cf. ibid.*, Sect. 190, p. 201, note 3.

[6] *Cf.* Koffka's criticism of this 'Dualismus': 'Psychologie,' in Dessoir's *Lehrbuch*, pp. 527*ff*.

iv. The psychophysics of the *Gestalt*-psychologist has next to be subjected to a critical regard. In opposition to the usual view that "the [physiological] substratum of sensation (and image) is . . . the arousal of a separate and circumscribed area of the cortex, while the substratum for association is the neural connection established between such areas,"[1] he has certainly shown not only that physical 'structures' occur but also that the psychophysical correlates of our experiences may very probably be structures of this type. The *Gestalt*-psychologist, however, tends to go further than this and to assume, unargued, a precise metaphysical parallelism. In Helson's clever phrase, he turns "the problem . . . into a postulate and the riddle is solved."[2] So, for example, Köhler heads a chapter "'Denn was innen, das ist aussen,'"[3] and Koffka says, "We maintain that the physical processes which underlie the structural phenomena *must* themselves possess the character of structures"[4]—a statement unsupported either by direct observation or by necessary inference. A lurking metaphysical bias is certainly suggested in a closely following paragraph in which Koffka asserts that "the closest analogy, in its essentials even an identity, exists between our psychological description and our physico-chemical deduction." He carries the thought further in a later paper.[5] "We cannot," he says, "cut off conscious processes from processes not accompanied by consciousness. We must assume that conscious processes are part-processes of larger wholes and that, by pointing to other parts of the same whole, they give us evidence that the physiological process is just such a whole as the mental one."[6] These dogmatic statements challenge the attention of the critical reader. Certainly, the configurationist has no right to argue from the fact that both psychical and physical configurations occur, that they precisely correspond, and still less to argue that both are parts of a 'larger whole.'

[1] Koffka, K., PSYCHOL. BULL., 1922, 19, p. 535[4].
[2] Helson, H., *Amer. J. Psychol.*, 1925, 36, p. 359[4].
[3] 'Die physischen Gestalten,' Abschn. IV., Kap. 1.
[4] Koffka, K., PSYCHOL. BULL., 1922, 19, p. 550. Italics mine.
[5] *Ibid.*, p. 552[2].
[6] Koffka, K., *Brit. J. Psychol.*, 1924, 15, p. 160[2].

v. A final comment concerns the novelty which *Gestalt* theorists attribute to their doctrine. Koffka's assertion that "in 1912, Wertheimer stated for the first time the principles of a *Gestalt-Theorie*" [1] is typical of their attitude; though it is only fair to quote, as supplementary statement, his remark that "the new psychology has not grown up out of nothing." [2] The configurationists' persuasion that they are introducing a radically new psychology finds expression, in the first place, in their tendency to stress the contrasts, rather than the likenesses, of contemporary systems with their own. For example, they rightly criticise the behaviorist on the ground that "the activity bits composing, in enormous quantity, the bulk of the behaviorist's studies, are presented as so many independent, isolated, elementary actions and not, as they ought to be, as part-processes of an embracing whole." [3] But when, by way of showing up the inadequacy of the behaviorist's view, Koffka goes on to say that "the description 'the animal is running away from some danger,' however insufficient it may be, is yet a thousand times more characteristic . . . than a formula giving us the movements of all its legs with their varying speeds," he unfairly obscures the fact that this is the precise contention of the behaviorist himself.[4] The criticism should have been levelled not at behavioristic theory but at the behaviorist's inconsistency with his own doctrine, his failure to play his own game.

A similar comment may be made on the *Gestalt*-psychologist's attitude to what has been called personalistic (or self-) psychology, a doctrine shared in more or less detailed agreement, though not always under this name, by James Ward, W. Mitchell, W. Stern, R. M. Yerkes, and many others. This doctrine conceives psychology as science of self (or

[1] Koffka, K., PSYCHOL. BULL., 1922, 19, p. 531[1].

[2] Koffka, K., 'Psychologie,' in Dessoir's *Lehrbuch*, p. 498[2].

[3] Koffka, K., 'Introspection and the Method of Psychology,' *Brit. J. Psychol.* 1924, 15, p. 160[3].

[4] *Cf.* Watson's contrast of psychology, as study of the reactions that the totally integrated individual, the 'whole organism,' makes to 'its environment as a whole' with physiology, as concerned with 'the functions of the special organs.' ('Psychology from the Standpoint of a Behaviorist,' pp. 13[2], 19[3]*f*, *Cf.* pp. 8, 12, 48, 193[3], 209 end.)

person) in reaction on its environment, social and physical.[1] Accordingly, it closely resembles the *Gestalt*-psychology, both negatively in its rejection of atomistic doctrine, and positively in that *Gestalt*-psychology also, as has appeared, invokes the experiencing individual. Configurationists, however, wholly ignore this group of their fellow-rebels against psychological atomism.[2]

Configurationists, not without some reason, justify this stand-offishness on the ground, already suggested, that behaviorists and self-psychologists, apperceptionists and upholders of the 'special activity' theories—in a word the protagonists of contemporary systems which are held to resemble *Gestalt*-psychology—either explicitly or implicitly admit the occurrence also of the separate mental elements,[3] thus adhering to the doctrine against which *Gestalt*-psychology is explicitly arrayed. Configurationists cannot, however, so plausibly allege this reason for ignoring certain specific anticipations of their central doctrine of perceptual configuration. George Humphreys has called attention to one of these, the Erdmann-Dodge 'Untersuchungen über das Lesen,'[4] which in

[1] *Cf.* James Ward's statement: The standpoint of psychology is that 'of the living subject in intercourse with his special environment.' (*Brit. J. Psychol.*, 1904, 1, p. 17.) It would be beside the main purpose of this paper to bring forward, at this point, the important distinction between personalists according as they conceive the person, or self, as 'conscious organism' or as 'self which has a body.' (*Cf.* a paper by the writer, on 'Converging Lines in Contemporary Psychology,' *Brit. J. Psychol.*, January, 1926.)

[2] The only exception, known to the writer, is the highly unfavorable review by Koffka (*Zsch. f. Psychol.*, 1912, **62**, pp. 218*ff.*) of the first edition of her 'First Book in Psychology' which, thirteen years later than it was published, she has just read. Koffka treats her much as he has more recently treated the behaviorists. He attacks her for the 'clearest imaginable atomism' in specific portions of the book—in her discussion of mental elements, of association, and kindred subjects. In many of these criticisms he is justified, though he doubtless sometimes mistakes for an atomistic conception what is merely an analytic procedure. The significant point is, however, that Koffka here again ignores the emphasized basal conception of the book with which the objurgated atomism is completely at variance.

[3] *Cf.* especially Koffka's treatment of 'der in der Wahrnehmung konstruirte Dualismus der höheren und niederen Prozesse' ('Psychologie,' in Dessoir's *Lehrbuch*, pp. 526*ff*). For a critical discussion of the relation of *Gestalt-Psychologie*, in the narrowest sense, to von Ehrenfels, Meinong, the 'Graz school,' the Würzburg group and allied writers, *cf.* H. Helson, *Amer. J. Psychol.*, 1925, **36**, pp. 360*ff*.

[4] For footnote references to this work by *Gestalt*-theorists, *cf.* Wertheimer, M., *Zsch. f. Psychol.*, 1912, **61**, p. 182, *et al.*

1898 argued experimentally for the main position of the *Gestalt-Theorie*. And no student of William James can have forgotten either the vigor with which he hews away, root and branch, at the current concept of sensations as 'composing' objects of consciousness or the persistence with which he enforces the concept of perception and of thought as apprehension of unified objects. The remainder of this paper, setting aside any similar anticipations of *Gestalt*-psychology, is devoted to the exposition of the James doctrine of configuration.

The opposition of William James to what he, like the configurationists, calls psychological atomism, comes out most strongly in his criticism of mechanical associationism and in his doctrine of sensation as related to perception. In the chapter on 'The Mind Stuff Theory' he condemns the "inwardly quite unintelligible . . . assumption that our mental states are composite in structure, made up of smaller states conjoined." [1] He carries the criticism into more detail in the later chapter on 'The Stream of Thought': "The ordinary associationist-psychology," he there points out, "supposes . . . that whenever an object of thought contains many elements, the thought itself must be made up of just as many ideas, one idea for each element, and all fused together in appearance but really separate. . . . I contend," he protests, "that *there is no manifold of coexisting ideas. . . . Whatever things are thought in relation are thought from the outset in a unity, in a single pulse of subjectivity, a single psychosis, feeling, or state of mind.*" [2] In the 'Sensation' chapter he renews his "struggle against the view that sensations are *immutable psychic things*" [3] and refers to the concept of the sensation "as a fractional *part* of the thought, in the old-fashioned atomistic sense" as a doctrine which he has "often criticised." [4] "We ought not," he over and over again asserts, in different contexts—"we ought not to treat the perception

[1] James, W., 'Principles of Psychology,' 1, p. 145.
[2] *Op. cit.*, 1, Chap. 9, pp. 277*f*.
[3] *Op. cit.*, 2, p. 27[3]. Italics mine.
[4] *Op. cit.*, 2, p. 5[1]. *Cf.* p. 2, Note; and p. 45[3].

as a sum of distinct psychic entities. . . . The perception is one state of mind or nothing."[1]

It is of interest to notice that James clinches this position by the use of an exact parallel, though from another sense-mode, of the *Gestalt*-psychologist's puzzle-picture argument. "Take," James says, "the . . . catch, *Pas de lieu Rhône que nous*. . . . As we seize the English meaning . . . (*paddle your own canoe*) the sound itself appears to change."[2] In other words, though the sensational elements remain the same, the second percept (as configuration, or unified whole) differs from the first.

A final unacknowledged agreement of Koffka with James has to do with the 'Psychologist's Fallacy,' as James calls it, to which he attributes the pernicious habit of taking percepts as mere combinations of sensations. "To a child," he says, "the taste of lemonade comes at first as a simple quality. He later learns . . . to perceive separately" (James must mean 'distinctly') "the sourness, the coolness, the sweet . . . all of which appear to be the elementary sensations into which the original 'lemonade flavor' is decomposed. . . . It is argued from this that the latter never was the simple thing which it seemed." But "the mind of the child enjoying the simple lemonade flavor and that of the same child grown up and analyzing it are in two entirely different conditions."[3] This is essentially what Koffka means when he says that "the same external situation may furnish phenomenal contents which are entirely different"[4] and again that "in many cases it is possible . . . to get meaningless sensations in the same situation that has already presented us with a meaningful, unitary whole."[5]

This emphasis on the important likeness between *Gestalt*-psychology and that of William James need not blind one to the differences between them. In particular, James's conception of the space-element "discernible in each and every

[1] James, W., 'Principles of Psychology,' 2, p. 80.
[2] *Ibid.*, 2, p. 80[2].
[3] *Op. cit.*, 2, p. 2, Note.
[4] Koffka, K., 'The Growth of the Mind,' p. 128[1].
[5] Koffka, K., *Brit. J. Psychol.*, 1924, 15, p. 154[1].

sensation," [1] and his conception, already referred to, of elemental 'transitive states of consciousness' ('feelings' of *and* and *but* and *if* occurring as certainly as the feelings of *blue* and of *cold*), are to the *Gestalt*-theorists anathema.[2] The psychophysical configurationist has probably scented, also, in the passage quoted from Chapter IX of 'The Principles of Psychology' a reference to the 'elements' of an 'object of thought' which suggests that James, whatever his psychological position, did not apply the *Gestalt* conception to the physical world.[3] Yet there are not lacking repeated statements which challenge this conclusion. Two of them follow. "A pure sensation," James says, "is an abstraction. . . . Any quality of a thing which affects our sense-organs does also more than that; it arouses processes in the hemispheres which are due to the organization of that organ by past experiences, and the results of which in consciousness are commonly described as ideas which the sensation suggests." [4] Here the word 'organization' has a *Gestalt*-like implication. More unambiguous is the statement, a little further on, "All brain processes are such as give rise to what we may call Figured Consciousness. If paths are irradiated at all they are irradiated in consistent systems and occasion thoughts of definite objects, not mere hodge-podges of elements. Brain paths . . . once excited together thereafter tend to vibrate as a systematic whole." [5]

Only the briefest of concluding paragraphs is necessary to draw together the threads of this discussion. The first division of this paper sets forth, in highly condensed form, the essential content of *Gestalt*-psychology. Part II presents the considerations on which it is based—summarizing, in particu-

[1] James, W., 'Principles of Psychology,' 2, pp. 134, 135[3]*ff.*

[2] It should be noted that James, the arch-eclectic in psychology, treats some of these 'feelings of transition' in a quite different fashion as subjective attitudes. *Cf.* his reference (*op. cit.*, 1, p. 251[1]) to 'attitudes of expectancy.' Similarly Koffka, in 'The Growth of the Mind,' p. 102[2], speaks of 'the expectant attitude.'

[3] *Cf.* also *op. cit.*, 2, p. 144, Note.

[4] James, W., 'Principles of Psychology,' 2, p. 76.

[5] *Ibid.*, pp. 82[3]*f.*

lar, the methods and results of experimental studies in perception and of observations of animal behavior. The concluding section of the paper, starting out from an expression of its author's agreement with the basal doctrines of *Gestalt-Theorie*, offers certain criticisms of its procedure and assumptions.

DISCUSSION

MOTIONS? OR EMOTIONS?

The work of Professor Albert Paul Weiss, entitled 'A Theoretical Basis of Human Behavior,' forms a valuable addition to the growing literature of the Behavior controversy, and the reviewer takes pleasure in noting its many and conspicuous merits. It is strictly up-to-date, issuing from the very blaze of the battle. Of the two hundred entries in the bibliography, only about ten go back to the first decade of the present century, besides the classics of Darwin, James, and Lange. The rest, near fifty books and one hundred forty articles, are gathered from the last fourteen years (1911–1924) and with a few notable exceptions, like Bechterev, Bernhard, Mach, Semon, Sergi, but not Köhler or Koffka, are all English and mainly American,—not unnaturally in view of the origin and development of the doctrine considered. Still more notable is the confident boldness of the author, his utter honesty and openness in the avowal of his principles, his courage in the face of contradiction and consequences sometimes regarded, if not evaded, as unpleasant or exposed to hostile prejudice. There is no shrinking or blinking in the Ohio Professor. He knows precisely where he stands; he feels assured his position is impregnable; and he would have the whole world know what issues are at stake, and how the contestants are ranged and directed. His knowledge of the matters in hand is exact and exhaustive, his interest therein is intense and enthusiastic, even to the point of wrecking, though rarely, his grammatical syntax; his logic is vigorous and incisive; his style is clear, manly, straight-forward, and unequivocal. Seldom does one need to reread a clause, sentence, or paragraph, to ascertain (what some high-and-dry traditionalist might call) its meaning, its bearings in the context. Considering the nature of its theme, one finds the volume in general very easy reading; the current of thought sweeps one swiftly and gladly on through an interesting, if not a fascinating, region.

What then is the message of Professor Weiss? What task has he undertaken? From his own point of view the title of his book

"If you remove the purpose of the mind, the bodily act is but as rotten wood."—Fo-sho-hing-tsan-king.

seems well-chosen, though an old-timer might possibly misunderstand it and look for some principle laid down as the basis of human conduct, like Kant's 'Categorical Imperative,' or the Golden Rule, or Li-R's maxim about 'repaying injury with kindness.' Nothing of the sort is in our author's mind (*sit venia verbo*). In one word, his message is mechanism. He would outline a view of the World as culminating (so far as 'known') in Man, more precisely, in Man's Behavior. This latter is not indeed the goal of the universal process, but only the frontage thus far attained. The world-temple appears to him as rising from solid foundations sunk below our ken, storey on storey, in steadily increasing complexities, up, up, to the vertex of socially organized Humanity. But though the structure 'evolves' from extreme simplicity to inconceivable complication, the building units remain the same throughout. And what are they? Electron-protons—unimaginably minute planetary systems of negative electrons spinning round positive protons. They congregate into atoms (of this or that type), and these into molecules, and these into compounds higher and higher,—for 'the great fleas themselves in turn have greater fleas to go on, and these again have greater still, and greater still, and so on'—until as spire and dome we behold Man in Society! But the only building units from first to last are the electrics.

What, then, is it that happens, that takes place? It is *movement*, nothing more, nothing less,—a dance of proto-electrons—at the base comparatively simple and uniform, at the top incomprehensibly if not infinitely convoluted, nevertheless a dance, that is all. Nor can it conceivably ever become anything else. If to dance is all any sub-atom can do, then to dance is all that a million or decillion or decillion cubed can do.

It is the office of Mechanics to describe this waltz, to tell how it goes on, to tell 'exactly and in the simplest way' (Kirchhoff). Herewith we assume that it goes on regularly, with more or less precise self-repetition ('Wenn im Unendlichen dasselbe Sich wiederholend ewig fliesst'). Our author indeed dwells on the notion of 'cause' or at least the 'causal' connexion of each with all, but he has the correct notion that any cross-section of history right across the time-axis, is at once the effect of all such sections before and the cause of all that follow (p. 48), though Mechanics knows naught of any causative influence or transit from one section to another. Such sections follow along like the terms of a compact series, but each simply is, it does not make nor unmake any other.

Much to his credit, our author insists on this purely mechanical view of the universe. Many whose premises imply as much yet shrink from stating the necessary conclusions. Kantor speaks of the 'infamous vitalistic and mechanistic theories' (xvi.), though it be hard to find in his 'Principles of Psychology' any recognized world-element that does not resolve into merely mechanical mass and motion. Professor Weiss, trained in the rigorous methods of his great teacher, Professor Meyer (to whom his book is dedicated), does not hesitate to emphasize the thorough-going mechanism of all things. For him History could seem to be a four-dimensional geometry, a kinematic or phoronomic. He makes bold to define an Individual (man) accordingly "on the basis of physical monism . . . as a locus in this movement continuum, and a function (in the mathematical sense) of the changes occurring in all other electron-proton aggregates" (p. 52, and even more explicitly, p. 392). Thus the individual is simply the *where* certain highly intricate, relatively systematic, and persistently cyclic, motions take place. You ask, 'But what moves'? The answer, not formally given by Weiss, can hardly linger: It is mass that moves, mass only. 'But what is *mass*?' Again we answer (as shown in the present writer's unpublished essay, 'Metastereoscopy,' 1922), it is space-curvature varying from point to point. Could we stand outside our 3-space in 4- or better in 5-space, on the time-axis, and behold the 3-space, the pulsating time-section of the 4-space (like looking at a varying curve from outside its plane), we should see it splotched all over, here densely, here sparsely, with countless flexures and undulations, like a sea-surface or the side of a mountain. Each more or less minute curved space-region would be a mass-body, be it sub-atom, atom, molecule, tree, animal, planet, or star, and all would be endlessly entangled in motion, like the 'deep-eddying ocean' in a storm. Each eddy would be an 'individual', more or less definite and persistent. The whole of 3-space would flutter as it sped along the time-axis, like a flag in a breeze, and that flutter would be the cinema of history.

Such seems to be the worldview, explicit or implicit, of our author, who does well in dwelling upon it, for all his following discussions, whether biographical or biosocial, must take their shape from the central basic conception. The problem, then, that must at once confront him, a problem he meets with undisturbed composure, is to state and classify the facts of human life in terms of physical movement, to express the whole of human experience

as a complex of mass-motions (whatever mass may be, for he is not responsible for the space-curvature interpretation suggested above). Certainly no light task, even for such as the Ohio Professor. Into the details of his constructions it is of course impossible to enter here, to anticipate the feast of reason, if not the flow of soul, that awaits the reader of these 400 beautiful pages. Sufficient to state the reaction of the present writer's own mind (?) and his general attitude in the presence of this virile volume.

It must already be clear that he sympathizes in large measure with the theses of the book in hand. For him, too, the (physical) world has long been a movement-continuum, to be understood and described in differential equations or their equivalents (without any prejudice to quantum-theory), a closed system without any inroads, from outside or inside, of non-physical nature. Surely this is going far along the path of Weiss and his peers, a path that looks at so many points so perfectly safe and familiar—quite as far in fact as even Weiss can go, the *whole* way in truth, since Weiss recognizes *nothing* beyond this continuum. But it remains none the less impossible for him to state or interpret all or *any* 'conscious' experience in terms of mass and motion, either or both. The physical facts exploited so freely by Behaviorists he recognizes fully and accepts at their face-value, along with a strictly mechanistic or kinematic conception of the phenomenal universe. So far, so good. Yet none of this implies or even suggests repudiation of any psychical fact, or its forcible attempted discerption into physical elements of mass and motion.[1] Granted (at least for argument's sake) that the physical facts are all precisely as Behaviorists claim; then they are indeed very much, but is there any hint that they are *all?* Does the Razor of Occam shear off the suspicion that there may be some *meaning behind them?* that it is not all *within* them? Here are two lines crossing at right angles at o, about which point as center lies a circle in their plane—a mere geometric datum of more or less interest. So far we are all agreed. There is not necessarily anything more. But *may* there not be something more? That circle so drawn or lying *may* be a sign or symbol, the geometric

[1] Such as Professor Weiss's, for he declares (p. 234): "The behaviorist concludes, that if mental or *conscious* processes are regarded as particular types of chemical or physical processes of as yet unknown composition, then only one entity or one system of events need be assumed"—which goes squarely against his teacher, Professor Meyer, who prefers (in 1908) "to state an hypothesis in mechanical rather than in other terms," but "does not share the views of those who believe that science must reduce the whole world to matter and motion." PSYCHOL. REV., 1908, 15, p. 300.

picture of the equation X^2 plus Y^2 equals 25. There is no resemblance whatever between circle and equation, yet either may be taken as *Abbild* of the other.

This almost excessively simple illustration makes clear that there *may* be symbolization or representation of one thing by another totally different. Exactly such a state of case seems to hold for the physical and the psychical; the former the symbol or depiction, the latter the original; the former the sign, the latter the significate. It seems to be the nature of Psyche (whoever she may be), to project, *vorstellen*, depict, objectify her states at every moment to form a steady continuum of such symbols, the physical world. Each facet or phase of the one universal Psyche builds up continually its own world around it, varying from instant to instant, but the countless series of such constructs (A, B, C, D, . . .) by the countless facets (*A*,*B*,*C*,*D*, . .) are so nearly alike (since so are *A*, *B*, *C*, *D*, . . .) that we count them identical and speak of the *one* physical world, instead of the multiple series of such worlds.

But why such an extravagant theory? Why suppose any such symbolization? Why not honestly accept the facts at their face-value? Answer: We *do* accept facts at their face-value with all possible honesty and deference. But another body of facts quite as impressive we must also accept, which can not be translated into any semblance of things physical.[1] No skill or ken can state the simplest psychic datum in terms of mass-motion, not even one in countless thousands. All attempts, the boldest and shrewdest, are obvious failures; they explain everything—except the only thing that calls for explanation. No fault of the Behaviorist; it lies wholly in the nature of the case. Psychic facts are no more facts of mass and motion than the pair of values (3, 4) is a point of a circle X^2 plus Y^2 equals 25. The one depicts the other, but is not the other nor ever can be. Hence any and all attempts to state the psychical in terms of the physical are like trying to cut the logical ties in a syllogism with a pair of scissors.

We need not insist on any question of *value*. It is obvious that in and of itself the movement-continuum, the whirl of atoms, is of no worth whatever, either to itself or to anything else; nor indeed can we attach any meaning to a statement that one movement *feels* interest in any other. No one supposes that the parts of a 'movie' have the slightest mutual concern. The physical world

[1] Professor Charles Henry recognizes this, though he tries to evade it in the bizarrest fashion.

seems to be just a gigantic motion-picture, in and of itself entirely void of any value, however immensely significant as a *symbol*. Professor Weiss seems to find no hint of superiority of one 'locus' to another save in the "optimum relationship between the innate and acquired components in the behavior of the individual." This 'optimum' is used six times in 180 words (p. 126 *f*) but remains formally undefined. It is impossible, however, to escape the notion of 'happiness,' and so we read: "Neurologically, the word happiness indicates the extent to which the innate and acquired components of sensori-motor function approach an optimum relationship between the antagonistic processes of individualization and socialization so that the movements of the individual are contributing directly or indirectly to larger or more complex electron-proton aggregates or larger and more complex social organization." We are reminded of a dozen deacons marching down the aisles at a church collection. Seriously, however, the author seems to *feel* that this is not the *whole* story, for he says 'Neurologically'! Some may think 'happiness' is mainly a matter of 'nerves,' but surely the nerves are neither sad nor glad; they are merely 'thrilling,' moving this way and that, like pieces on a chess-board, no more, no less. It is something else that is happy or unhappy, something that *feels* and is no part of any movement-continuum. Notice also that Professor Weiss is quite logical. He knows that the 'Loci' in this continuum can differ only in size and complexity, hence the 'relationship' is 'optimum' only as the 'movements' contribute most to larger and more complex 'electron-proton aggregates' or 'social organization.' Granted that such electric 'aggregates' and 'organization' may *symbolize* happier and even better feelings and thinking conditions, yet it is to these latter only that the terms goodness, happiness, and the like can apply. Betelgeuse and Antares (even Behemoth and Leviathan) are no better and no happier for being so large; in themselves they are neither good nor happy nor beautiful nor awesome (*pace* Kant), though they may well symbolize such-named mental activities. A glowing fire is not comfort nor comforting, though it may depict partially an enjoyable sensual psychic condition. Neither is any sensori-synaptic-motor activity either pleasant or unpleasant, though it may well be what Professor Meyer calls aptly the 'Nervous Correlate of Pleasantness and Unpleasantness.' So too the pound-note is the correlate of a certain exchange-value, but it is not that value itself.

Lastly, it may be asked, what right have we to assume any

such one-to-one correspondence, and especially that the physical is the token and product of psychic activity? The answer is that the facts of life leave us no choice. In particular, sensation and perception, no less than conception, imperiously demand the assumption. Most obviously, the 'baffling phenomena' (as Whitehead calls them), the raw material lying on the fringe of Experience, such as refraction, reflection in a mirror, double-vision, the after-image, the rainbow, dreams, visions, hallucinations—all these, so rarely named by Behaviorists, defy interpretation as merely facts of motion. In all these the Psyche is caught *flagrante delicto*, in the very act of *making percepts in perceiving*.

What is the place or sense of Error in a movement-continuum or electron-proton aggregate? The ablest Realists have long felt and avowed that herein lies the crucial problem and accordingly have devoted their utmost powers to the search for some solution—with a purely negative result, as seen in the subtle 'New Realism 'of the Six Thinkers; no one is able to convince another. Moreover, the past-master of both Mathematics and Philosophy, Bertrand Russell, has admitted that the objects of sight 'probably do not exist' when the observer no longer looks at them, that the 'sensible objects of waking life must not be expected to have any more intrinsic reality than those of dreams'—admissions no less decisive because exceeding cautious.

Our thanks and admiration are no less due to Professor Weiss, that he does not attain the goal of his endeavors: *In maximis tentavisse sat est.*

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